DIVERGENT SERIES AND ASYMPTOTIC EXPANSIONS, 1850-1900

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Abstract

Divergent series enjoyed a period of free and easy use in the eighteenth century. This was somewhat curtailed in the early nineteenth century even though interesting applied problems were solved using asymptotic expansions of divergent series during that time. In the second half of the nineteenth century asymptotic expansions were placed on a firm and rigorous foundation such that their use become standard practice by early in the twentieth century. I elucidate how and why that happened by examining the work on asymptotic expansions of George Gabriel Stokes, Jules Henri Poincaré and Thomas Jan Stieltjes.

Keywords: Divergent series; Asymptotic expansions; Stokes; Poincaré; Stieltjes

For Nadia and Stefan

"Abel wrote in 1828: 'Divergent series are the invention of the devil, and it is shameful to base on them any demonstration whatsoever.' In the ensuing period of critical revision they were simply rejected. Then came a time when it was found that something after all could be done about them. This is now a matter of course, but in the early years of the century the subject, while in no way mystical or unrigorous, was *regarded as sensational, and about the present title [Divergent Series], now colourless, there hung an aroma of paradox and audacity"*

— *J.E. Littlewood*, DIVERGENT SERIES, *1948*

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Introduction

Consider a function, represented in its infinite series Taylor expansion. For example:

$$e^x = 1 + x + \frac{x^2}{2!} + \frac{x^3}{3!} + ... = \sum_{n=0}^{\infty} \frac{x^n}{n!}$$

It is possible to approximate the value of e^x for any value of x by using the first n terms of the series and then truncating and ignoring the rest of the terms. This procedure produces an approximation that has an error associated with it.

In the case of this example, the infinite series for e^x converges for all values of x, resulting in an approximation with increasingly less error as n increases. However, depending on the function and the representative infinite series, the series may converge for only limited values of the argument, x, or it may only converge for x equal to zero. Convergent series are well understood, with well defined methods of bounding the error of the approximations made using truncated infinite series.

Divergent series, on the other hand, are more difficult and more interesting. A canonical example of a function and its associated divergent infinite series representation is the Stirling series for the factorial function,

$$n! \sim \sqrt{2\pi n} \left(\frac{n}{e}\right)^n \left(1 + \frac{1}{12n} + \frac{1}{288n^2} - \frac{139}{51840n^3} - \frac{571}{2488320n^4} + \cdots\right).$$

The infinite series on the right diverges for all values of n. However, a good approximation for $n!$ can be obtained by using a carefully selected number of terms of the divergent series. Furthermore it is possible to bound, but not to make arbitrarily small, the error in the approximation even though the series is divergent.

The Stirling series representation for $n!$ was formulated by Abraham De Moivre (1667-1754) and improved upon by James Stirling (1672-1770). The series was shown to be divergent by Thomas Bayes (c. 1702-1761) in a letter published in 1763. For a brief history of the Stirling series see (Roy 2011, p.476-486). This type of series when truncated after a given number of terms is now called an asymptotic expansion of the function, or an asymptotic approximation to the function. I think it is fair to claim that nearly all mathematicians by 1800 were familiar with this example and that it caused confusion and consternation along with its obvious utility.

Over time there have been several different terms used for what is now called an asymptotic expansion. Among the terms that have been used are: descending series, half-convergent series, and semi-convergent series. Further, and particularly for the term semi-convergent, these terms have been used ambiguously and inconsistently. As the terminology does not bear on the subject of this thesis for the most part, I have noted, but not examined in detail, the different terms used by the various authors of the work analyzed in this thesis.

The utility of asymptotic approximations during the eighteenth and early nineteenth centuries — efficient computation, in particular — could not be ignored, particularly in an era when computations were done by hand. It was, however, not clear at this time why these approximations worked so well, or what the ultimate divergence of the series meant for the approximation. By roughly 1920 the mathematics of divergent series had been extensively studied and the theory of asymptotic expansions had been placed on a rigorous footing. John Edensor Littlewood (1885-1977) nicely summarized this change in the preface of a 1949 textbook on divergent series when he said:

> "This [the use of divergent series for approximation] is now a matter of course, but in the early years of the century the subject, while in no way mystical or unrigorous, *was* regarded as sensational, and about the present title [Divergent Series], now colourless, there hung an aroma of paradox and audacity." (Hardy 1949, p.viii)

In this thesis I examine the use of asymptotic expansions of divergent infinite series during the second half of the nineteenth century with a focus on the mathematical work of three people: George Gabriel Stokes (1819-1903), Jules Henri Poincaré (1854-1912) and Thomas Jan Stieltjes (1856-1894). I claim that it is primarily the work of these three individuals that propelled the transformation of divergent series from what they were in 1763 to what they became in 1920.

I claim that, during the mid-nineteenth century, divergent series were successfully used by George Gabriel Stokes for two purposes: to verify new physical theory and to generate numbers from existing theory. Further, I claim an important change in attitude toward divergent series, which had up to that point been used both knowingly and unknowingly and with contested authority, underlies the work of Stokes.

The success of Stokes' use of divergent series did not immediately translate into the widespread use of divergent series in Britain or elsewhere following the publication of his work in 1856. I claim that the next important work on this topic was that of Stieltjes and Poincaré, both of whom published on this topic in 1886.

The work of these three individuals, and particularly the work of Poincaré, set the stage for the pure mathematical work in divergent series that followed — both that of asymptotic analysis and that of summability theory. The work analyzed in this thesis was the genesis of a complete, rigorous, mathematical understanding of divergent series — work which achieved maturity in the work of Borel in the 1920s. The scope of this thesis is confined to asymptotic expansions of divergent series and does not analyze the genesis of summability theory.

As we shall see, the use of divergent series to provide asymptotic expansions for computational purposes as well as for the purpose of understanding of physical phenomena changed significantly between 1850 and 1890, such that the use of this tool was better understood, brought into line with the current standards of rigour and was employed in different contexts by several practitioners. This set the stage for the widespread use of asymptotic expansions in the twentieth century.

Many authors have written about divergent series and related topics and I rely on both primary and secondary sources to provide both a condensed summary of the history of the use of divergent series up to 1850 and to deepen my understanding of the change in the use and understanding of divergent series that occurred in the second half of the nineteenth century. Further I have used several sources to add cultural context and to understand the motivation behind these changes.

To set the stage for the changes that came in the second half of the nineteenth century, I provide a summary, in this introduction, of what came before 1850. This summary relies heavily on the work of Morris Kline (Kline 1990), Heinrich Burkhardt (Burkhardt 1911) and Augustus De Morgan (1806-1871) (De Morgan 1849). These works speak directly to the understanding of divergent series at the middle of the nineteenth century. The 1844 paper of De Morgan is particularly good at capturing the British context at the time immediately prior to when Stokes worked on divergent series.

After establishing how divergent series were sporadically used in mathematical practice during the first half of the nineteenth century, I examine, in Chapter 2, an important cultural priority — that of precision — which surrounded the natural philosophical work being done in the first half of the nineteenth century in Britain. Until the nineteenth century, natural philosophy was the common term for the study of nature and this included what we now call physics.

A strong desire to quantify, measure and order was seen as important for science, for commerce, for industry, and for society. Important secondary literature I use to establish the importance of precision are the collective volume *The Values of Precision* edited by M. Norton Wise (Wise 1995) as well as the paper *The Calculating Eye: Baily, Herschel, Babbage and the business of astronomy* by William Ashworth (Ashworth 1994). I also quote several natural philosophers to show that they personally and professionally valued exactitude.

For two reasons, and partly in the context of precision, I analyze in detail the science of the pendulum and its use in understanding physical phenomena. The first reason is that the pendulum is an excellent example where precise measurements were required and those measurements were important, particularly in the British context. The second reason is that precise pendulum measurements led to the understanding of a previously unknown physical phenomenon. It is the precision measurements taken with the pendulum that allowed Stokes to discover the index of friction which is now known as an epiphenomenon of viscosity. Precise pendulum measurements also allowed Stokes to justify his asymptotic approximations of divergent series. A key source of information on the role and importance of the pendulum during the nineteenth century is the lengthy article, *The Development of Gravity Pendulums in the 19th Century*, by Victor Fritz Lenzen and Robert P. Multhauf (Lenzen & Multhauf 1966).

Further along in Chapter 2, I show why, how, and with what results Stokes used pendulum data to develop and refine hydrodynamical theory and to generate numbers from this theory. As we shall see, the attempt to match physical theory to the precise pendulum measurements of William Hallowes Miller (1801-1880) led to the physical discovery of the effect of viscosity and thus the boundary layer. It would not have been possible to generate the numbers from theory that resulted in this discovery without the use of asymptotic expansions — the computational complexity was simply too great using other methods.

In Chapter 3, I analyze the mathematics of divergent series that Stokes used and discuss the underlying, and sometimes unstated, assumptions in his work. As we shall see, the approach of Stokes may be best described as pragmatic. Stokes started with known convergent definite integrals whose values predicted physically measurable quantities. Without thorough mathematical justification, Stokes took those integrals, converted them into divergent series and then used the first few terms of those series to approximate the original integrals.

The mathematical justification for his method was based on qualitative statements about the differential equations from which the integrals arose. More important though, as we shall see, for the justification of his mathematics, was that the numbers that he obtained from theory accurately matched with experimental values produced in a laboratory.

I have not been able to identify anyone who expanded on the mathematics of divergent series that Stokes developed. I have not even been able to find much use of his method to solve contemporary problems — either with theory or with computational difficulties. As we shall see in the conclusion, there are a couple of textbooks that referenced Stokes' papers; one from 1899 and the other from 1908. The 1908 textbook also had a short section devoted to finding asymptotic expansions using the method of Stokes.

That the most significant work on asymptotic expansions was done about thirty years later is corroborated by Godfrey Harold Hardy (1877-1947) who said:

> "Divergent asymptotic series occur in the works of most of the older analysts, but the first mathematicians to make a systematic study of them were Poincaré and Stieltjes, and the first general theory is contained in a famous memoir of Poincaré on differential equations." (Hardy 1949, p.28)

I therefore move forward in time to look at the work of Poincaré and Stieltjes.

Chapters 4 and 5 are concerned with the work of Poincaré. We first see, in Chapter 4, that like Stokes, Poincaré's work was situated in mathematical physics. In the case of Poincaré, it was in the analysis and understanding of the differential equations of celestial mechanics that asymptotic expansions of divergent series arose. In Chapter 5, as was done in Chapter 3 with the mathematical work of Stokes, I examine in detail how Poincaré mathematically handled asymptotic expansions.

The work of Stieltjes is discussed in Chapter 6 where I show that his work is of a different character than the work of Stokes or Poincaré. Stieltjes had a different type of problem to solve — he was simply looking at how to find the values of infinite series. There was no obvious physical motivation for this. This episode is included partially because it happened in the same year as the work of Poincaré and also because there are some interesting parallels between the work of Stieltjes and Poincaré. It is primarily Stieltjes' and Poincaré's work that informed what followed.

Computation and tabulation of the orbits of the objects of the solar system was a major topic of mathematical physics during the entire nineteenth century. In the period between 1798 and 1825, Pierre Simon Laplace (1749-1827) published an important multi-volume work, *Traité de mécanique céleste* (Laplace 1798-1825), on celestial mechanics and throughout the remainder of the century, there were many papers published about astronomical orbits. The work by Philippe Nabonnand (Nabonnand 2012) titled *Les premières contributions de Poincaré en mécanique céleste vues à partir de sa correspondance avec Anders Lindstedt (1883-1884)* provides a history of developments of celestial mechanics in the nineteenth century with a focus on the work of Poincaré.

A summary article by George William Hill (1838-1914), *Remarks on the Progress of Celestial Mechanics since the Middle of the Century* (Hill 1896b) is the text of Hill's presidential address to the *American Mathematical Society* in 1895. This paper provides a summary of the mathematical progress in astronomy during the second half of the nineteenth century, again with an emphasis on the results of Poincaré near the end of the century. Hill also provided an opinionated list of the available (in 1896) histories of celestial mechanics and he claimed that "A thoroughly satisfactory history of our subject is yet to be written" (Hill 1896b, p.334).

By the early 1880s, Poincaré had turned his attention to solving the equations of celestial mechanics where there were two overarching questions: first, were the gravitational laws of Isaac Newton (1642-1726/27) sufficient to explain the motions of the heavenly bodies and second, was our solar system stable? The stability question became particularly acute following the recognition that the infinite series solutions of the differential equations of celestial mechanics were divergent. A partial answer to both these questions lay in the understanding of the three-body problem which is simpler, special case of the n-body problem that is used to model a solar system with n masses.

Poincaré first analyzed the three body problem in a paper published in 1886 (in his entry to the famed prize competition of 1889, to mark the sixtieth birthday of Oscar II, King of Sweden and Norway) and later in his large and enormously influential three volume work *Les méthodes nouvelles de la mécanique céleste* published between 1892 and 1899. June Barrow-Green's book, *Poincaré and the Three Body Problem* (Barrow-Green 1996) explains the history, importance and mathematics of this work. In Chapter 5, I analyze the mathematics of divergent series that Poincaré developed and compare and contrast that with Stokes' work.

Finally, in Chapter 6, I analyze the 1886 doctoral dissertation of Stieltjes, the topic of which is asymptotic expansions of infinite series. His thesis is an analysis of five infinite series, not directly motivated by mathematical physics, and their asymptotic expansions such that values can be assigned to the infinite series and that the error in the resulting approximations can be bounded. Some of these infinite series are important in number theory (for example the logarithmic integral) while others foreshadow Stieltjes' interest in the analysis of continued fractions.

1.1 Selected Topics in the History of Divergent Series to 1850

Before I look at the three important episodes in the development of the mathematics of divergent series I have based this thesis on, I provide here a short introduction to the use, to the misuse, to the justification for, and to objections to the use of divergent series in the period of time between Euler and 1850.

1.1.1 Euler and Earlier

Infinite series were used well before Euler's time, in India at the turn of the fourteenth century (see for example Ranjan Roy (Roy 2011, p1-10)) and then, in the European context, by John Wallis (1616/8-1703), Newton, Gottfried Wilhelm Leibniz (1646-1716), James Gregory (1638-1675), and Colin Maclaurin (1698-1746) among others. Roy, perhaps anachronistically, identified the beginning of the development of infinite series and products (convergent or divergent) with a new era in mathematics which started in the mid-seventeenth century. In his opinion,

> "The development of infinite series and products marked the beginning of the modern mathematical era. In his *Arithmetica Infinitorum* of 1656, Wallis made groundbreaking discoveries in the use of such products and continued fractions. This work had a tremendous catalytic effect on the young Newton, leading him to the discovery of the binomial theorem for non-integer exponents. Newton explained in his *De Methodis* that the central pillar of his work in algebra and calculus was the powerful new method of infinite series." (Roy 2011, p.xvii)

Hardy devoted the first 41 pages of his 1949 text on divergent series (Hardy 1949) to an introduction to and historical remarks about divergent series. Hardy claimed (Hardy 1949, p.5) that Newton and Leibniz were first to make systematic use of infinite series and, in the main, they kept to convergent series — a severely orthodox treatment according to Hardy. Further, in Hardy's opinion, Newton was the first to master this really powerful technique and as such there was much for him to do and the rewards of orthodoxy were sufficient.

There is an interesting exception to the above statement provided by Leibniz. Leibniz and Johann Bernoulli (1667-1748) engaged in a sixteen month correspondence, which began in 1702, about the value of $\log(-1)$. Bernoulli argued that the value was real and, in particular, zero, whereas Leibniz argued that the value was imaginary. Bernoulli's argument can be summarized as follows:

$$\frac{dx}{x} = \frac{d(-x)}{-x} \implies \int \frac{dx}{x} = \int \frac{d(-x)}{-x} \implies \log(x) = \log(-x)$$

Thus, $\log(-1) = \log(1)$ and the value is real and zero.

To this, Leibniz responded with three different arguments. The relevant one here is that Leibniz put $x = -2$ into the series expansion

$$\log(1+x) = x - \frac{x^2}{2} + \frac{x^3}{3} - \cdots$$

and then claimed that because the series on the right diverges, the value on the left cannot be real and therefore is imaginary (Kleiner & Movshovitz-Hadar 1994, p.966).

The idea of using series divergence, without finding an expansion or a sum, to make a decision about something as Leibniz did regarding $\log(-1)$ is uncommon. As we shall see in Chapter 6 though, Stieltjes also used series divergence in this manner to make a decision — in his case it was to determine that a related continued fraction converged.

Even though De Moivre published Stirling's formula in 1730 in *Miscellanea Analytica*, it is with Euler that the use of divergent series became a broadly useful tool. In 1745, shortly after his program to separate analysis from geometry, Euler investigated infinite series and, in particular, series that did not converge. Fraser and Schroter (Fraser & Schroter 2021) have identified Euler's paper submitted to the Berlin Academy in 1746 as his most productive effort on divergent series. And, for example, Hardy (Hardy 1949, p.23) took, as his first historical example, Euler's demonstration (in 1749) of the functional equation

$$(2^{s-1}-1)\eta(1-s) = -(2^s-1)\pi^{-s}\cos(\frac{1}{2}s\pi)\Gamma(s)\eta(s)$$

via use of the equality

$$\frac{1-2^{s-1}+3^{s-1}-...}{1-2^{-s}+3^{-s}-...} = -\frac{(s-1)!(2^s-1)}{(2^{s-1}-1)\pi^s}\cos(\frac{1}{2}s\pi)$$

where $\eta(s)$ is the Dirichlet eta function, also known as the alternating zeta function.

The series above are only convergent for $s = \frac{1}{2}$. Euler was aware of this and used the series when $s \neq \frac{1}{2}$ in the sense the sum was taken to be the limiting value of the series as the variable approached $\frac{1}{2}$.

Fraser and Schroter noted that this was part of a shift in the calculus from its geometric form to a form of algebraic analysis and this shift made possible this use of infinite series. They expressed it thus:

> "Under this philosophy, the general applicability of any method derived from the generality of its *object*. Since formulas were objectively given as part of algebra, their generality of usage was assured, even if this gave rise to divergent

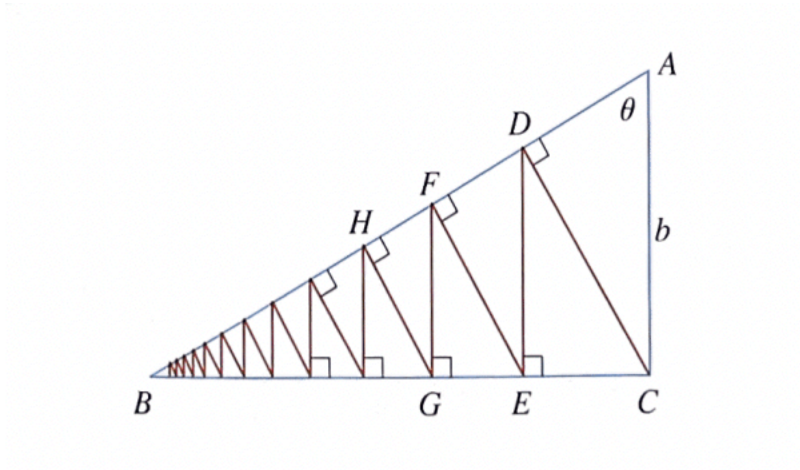

Figure 1.1: A geometric representation of an infinite series (Stewart 2015, p.718)

> series. Formalism thus became untethered from geometry while remaining subtly connected with intuitive notions of quantity. As the most prolific practitioner of this new analysis, Euler's willingness to pursue formalism's implications for infinite series brought this tension to the foreground." (Fraser & Schroter 2021, p.14)

This marked not only a shift to the use of formal power series without consideration of their convergence properties, but also a shift in the thinking about what formulae were capable of saying. Ferraro argued in the 2008 book, *The Rise and Development of the Theory of Series up to the Early 1820s* (Ferraro 2008), that the mathematicians who first used series had an intuitive idea of convergence and they were interested in using series to represent geometrical quantities.

This meant that the what the series represented could be drawn and was therefore a finite quantity — typically a length or an area. For example, consider computing the line length

$$|CD| + |DE| + |EF| + ...$$

as shown in Figure 1.1. The infinite series that represents this sum can be assumed to be convergent because the drawing appears to clearly indicate that the line length represented is finite.

With little distinction between finite and infinite series, similar methods were applied to both. However, because the infinite series used were almost always convergent these formal methods admitted little controversy. This balance between the quantitative and the formal was altered by 1720 in a way that emphasized formal manipulation without regard to the underlying object — that is whether it was finite, infinite and convergent or indeed infinite and divergent. This more formal use of infinite series allowed for the use of algebra to manipulate and generate results using divergent infinite series (Ferraro 2008, p.viii).

1.1.2 Between Euler and 1850

Hardy opined that Siméon Denis Poisson (1781-1840) and Jean-Baptiste Joseph Fourier (1768-1830) appeared to have been the mathematicians to make the most use of divergent series following Euler (Hardy 1949, p.17) and, whether this is true or not, they both certainly contributed extensively in this domain. In a paper written by De Morgan, which is carefully analyzed in what follows, we shall see the role Poisson played in more detail. By the early nineteenth century, the standard history (see for example (Kline 1990, p.1096)) is that there was a ban on the use of divergent series. This is often considered to have happened under the influence of Augustin-Louis Cauchy (1789-1857) and Niels Henrik Abel (1802-1829). Cauchy rigorously proved the theorems of the calculus in the early nineteenth century and this involved rejecting the heuristic principle of the generality of algebra. Abel's objection to the use of divergent series is clear in the following quote from 1826 taken from a letter that Abel wrote to his teacher Bernt Michael Holmboe (1795-1850). Abel said:

> "Les séries divergentes sont en général quelque chose de bien fatal et c'est une honte qu'on ose y fonder aucune démonstration. On peut démontrer tout ce qu'on veut en les employant, et ce sont elles qui ont fait tant de malheurs et qui ont enfanté tant de paradoxes...Enfin mes yeux se sont dessillés d'une manière frappante, car à l'exception des cas les plus simples, par exemple les séries géométriques, il ne se trouve dans les mathématiques presque aucune série infinie dont la somme soit déterminée d'une manière rigoureuse, c'est-à-dire que la partie la plus essentielle des mathématiques est sans fondement. Pour la plus grande partie les résultats sont justes il est vrai, mais c'est là une chose bien étrange. Je m'occupe à en chercher la raison, problème très interessant."[1] (Abel, Sylow & Lie 1881a)

[1]Divergent series are the invention of the devil, and it is a shame to base on them any demonstration whatsoever. By using them, one may draw any conclusion he pleases and that is why these series have produced so many fallacies and so many paradoxes ... I have become prodigiously attentive to all this, for with the exception of the geometrical series, there does not exist in all of mathematics a single infinite series the sum of which has been determined rigorously. In other words, the things which are most important in mathematics are also those which have the least foundation (Kline 1990).

Further, Ferraro identified an end point to the formal approach to series theory, as he saw it, with two early 1820s publications by Cauchy. Ferraro ended his book at the time when the eighteenth century formal approach to infinite series was perhaps no longer considered safe or at least had to be supplemented with convergence considerations. Ferraro said:

> "The point of arrival [i.e. the end of his book] is the early 1820's when Cauchy published *Cours d'analyse* and *Résumé des leçons données à l'École Royale Polytechnique sur le calcul infinitésimal* , which can be considered to mark the definitive abandonment of the eighteenth century formal approach to series theory." (Ferraro 2008, p.vii)

The reasons for the objections to the use of divergent series, typified by the remarks of Abel and Cauchy, were several. One substantial objection to the use of divergent series arose with the need to treat infinite series other than power series. In particular, the use, by Fourier and others, of trigonometric series raised the real possibility of introducing error through formal manipulation.

According to Ferraro (Ferraro 2007a), in the period 1770 to 1820, new objects such as the gamma function were being studied and successful results were often obtained by relying on geometrical or physical considerations which was in opposition to the Eulerian program in which analysis was independent of geometry. Ferraro claimed that this was the result of the exhaustion of the formal approach for finding new results (Ferraro 2007a, p.80-81). It was in this context that Fourier's 1822 treatise, *Théorie analytique de la chaleur* (Fourier 2009), and Carl Friedrich Gauss' (1777-1855) 1812 paper on the hypergeometric series were written (Gauss 1812). Both Fourier and Gauss highlighted the quantitative meaning of their results and rejected formal manipulations. Thus, the equality between a function and its infinite series had to be valid everywhere.

In spite of his stated objections to the use of divergent series, Cauchy continued to use them and he published a paper in 1843 titled *Sur un emploi légitime des séries divergente* (Cauchy 1843) in which he discussed an asymptotic approximation to the Stirling series which he used to compute $\log(\Gamma(x))$. In this paper, Cauchy confessed that he did not understand why the approximation was so good.

There are other examples of the incipient use of asymptotic expansions early in the nineteenth century. For example Laplace used asymptotic expansions just before and after the turn of the nineteenth century. Kline noted that Laplace, in 1812, used an asymptotic expansion to find values of the error function for large values of T (Kline 1990, p.1098).

The error function is defined by the integral

$$\int_T^{\infty} e^{-t^2}\ dt$$

and repeated integration by parts gives a divergent series for which the first few terms can be used to provide an approximation for large values of T. Even earlier and in the context of celestial mechanics, in 1790, Laplace provided an asymptotic expansion for the Legendre polynomials (named for Adrien-Marie Legendre (1752-1833)) later referred to by some as Laplace's coefficients. This expansion was, as we shall see in Chapter 6, analyzed nearly a century later by both Jean-Gaston Darboux (1842-1917) and Stieltjes (Szegö 1939, p.195-196).

Another example of the early use of what was later recognized as an asymptotic expansion was Legendre's approximation to the prime counting function. In 1798 Legendre approximated $\pi(a)$ by $\dfrac{a}{A\log(a)+B}$, where A and B were unspecified constants. Later in 1808, in the second edition of *Essai de la théorie des nombres*, Legendre provided values for A and B of 1 and -1.08366 respectively (Legendre 2009). As we shall see in Chapter 6, Stieltjes adopted Legendre's terminology for asymptotic expansions.

An important use of infinite series is the evaluation of definite integrals. Often a convergent definite integral can be evaluated using a convergent infinite series but, also often, the series converges too slowly for useful approximation. Evaluating convergent definite integrals using a divergent series instead, where the first few terms give an accurate approximation, is then highly desirable.

In the same manner that Fraser and Schroter identified Euler's use of divergent series as a marker for an underlying change in the perception of what the mathematics meant, Kline has argued that the difference in how divergent series were handled at the beginning and at the end of the nineteenth century is indicative of another shift in the thinking about what mathematics is. Kline stated:

> "Whereas in the first part of the nineteenth century they [mathematicians] accepted the ban on divergent series on the ground that mathematics was restricted by some inner requirements or the dictates of nature to a fixed class of correct concepts, by the end of the century they recognized their freedom to entertain any ideas that seemed to offer any utility." (Kline 1990, p.1096)

The Cambridge symbolists, whose most important member was George Peacock (1791-1858) (Pycior 1981), represented another school of thought during the first half of the nineteenth century. Their philosophy was that analysis consisted in working with a set of symbols on which operations were performed in accordance with certain laws. The operations on the symbols were divorced from any meaning the symbols might have and were therefore valid for all values of the symbols. Peacock defended the use of divergent series in this manner using what he termed the principle of permanence of forms. This was a philosophical statement about the generality of objects that are algebraically equal, which Peacock, as quoted by Pycior, stated as:

> "Whatever form is Algebraically equivalent to another, when expressed in general symbols, must be true, whatever those symbols denote. Conversely, if we discover an equivalent form in Arithmetical Algebra or any other subordinate science, when the symbols are general in form though specific in their nature, the same must be an equivalent form, when the symbols are general in their nature as well as in their form." (Pycior 1981, p.38)

Both Kline (Kline 1990, p.974) and Hardy (Hardy 1949, p.18) have stated that differences in national context affected the level of rigour that was required when handling series. The French were more likely to follow Cauchy and not admit calculation with divergent series whereas the British (partially under the influence of Peacock) and the Germans were freer with their use of divergent series.

The symbolical thought process espoused by Peacock came under criticism later. The arithmetization of analysis, which separated analysis from geometry, concerningly also separated analysis from intuition and physical thinking. Did this make analysis, in the words of German mathematician Paul David Gustav du Bois-Reymond (1831-1889):

> "A simple game of symbols where the written signs take on an arbitrary significance of the pieces in a chess or card game"? (Kline 1990, p.973)

1.1.3 The Status of Divergent Series in Britain at 1850

I am interested, in this thesis, in using the work of Stokes, Poincaré and Stieltjes to demonstrate that these three individuals were key to taking a topic that was disputed and transforming it into something that was generally applicable and mathematically acceptable. Applicability to mathematical physics (Stokes and Poincaré) and increased mathematical rigor (Poincaré and Stieltjes) are the two main themes and I conclude this introduction with a lengthy look at two papers which carefully analyze the state of divergent series in Britain in the time between 1840 and 1850 because it is in the mid-nineteenth century that this transformation began in earnest.

To discuss the British view of convergence and the use of divergent series during the mid-1840s, I analyze two papers in detail. Those two papers are:

1) De Morgan's paper *On Divergent Series, and various Points of Analysis connected with them* which was read on March 4, 1844 and published in Volume 8 of the Transactions of the Cambridge Philosophical Society in 1849.

2) Heinrich Burkhardt's (1861-1914) paper, *Über den Gebrauch divergenter Reihen in der Zeit von 1750–1860* which was written in 1911 and in which he carefully analyzed the British work on divergent series between 1840 and 1850.

The very first sentence of the De Morgan paper captures the unease and perplexity associated with the use of divergent series in the mid-nineteenth century. De Morgan said:

> "I believe that it will be generally admitted that the heading of this paper describes the only subject yet remaining, of an elementary character, on which a serious schism exists among mathematicians as to absolute correctness or incorrectness of results." (De Morgan 1849, p.183)

In his paper, De Morgan first differentiated between convergent and divergent series. He preferred to call divergent series non-convergent series, so as to distinguish between two types of non-convergent series — the first type which becomes infinite and the second type which stays finite but has partial sums that do not approach a limit.

According to De Morgan, only convergent series can be the objects of arithmetic calculation. Thus he had no argument with those who reject all non-convergent series. He was, however, particularly scathing of analysts who allowed the finitely non-converging series but disallowed the infinite non-converging series. De Morgan claimed that this was a common practice among analysts who objected to divergent series, both at home and abroad.

This was in contrast to what De Morgan understood of Euler, who freely used infinitely diverging series but considered finitely diverging series as indeterminate. De Morgan stated it thus:

> "The moderns seem to me to have made a similar confusion in regard to their rejection of divergent series: meaning sometimes that they cannot be safely used under existing ideas as to their meaning and origin, sometimes that the mere idea of any one applying them at all, under any circumstances, is an absurdity. We must admit that many series are such as we cannot at present safely use, except as means of discovery, the results of which are to be subsequently verified: and the most determined rejector of all divergent series doubtless makes this use of them in his closet. But to say that what we cannot use no others ever can, to

refuse that faith in the future prospects of algebra which has already realised so brilliant a harvest, and to train the future promoter of analysis in a notion which will necessarily prevent him from turning his steps to quarters from whence his predecessors have never returned empty-handed, seems to me a departure from all rules of prudence. The motto which I should adopt against a course which seems to me calculated to stop the progress of discovery would be contained in a work and a symbol — remember the $\sqrt{-1}$." (De Morgan 1849, p.182)

Thus De Morgan expressed the tension between the belief that arithmetic can only apply to convergent series and the reality that doing arithmetic with non-convergent series, even when one does not fully understand them, had provided the possibility of creating new knowledge. De Morgan asked if analysis would have developed as it did if Euler and others had refused to use $\sqrt{-1}$.

Following the introductory remarks from which the above quotes were taken, De Morgan's stated purpose, as expressed in his Section I title, was to show that all divergent series, either finitely or infinitely diverging, must be treated in the same manner. De Morgan said:

> "All Divergent Series, whether their divergence be finite or infinite, stand upon the same basis, and ought to be accepted or rejected together, as far as any grounds of confidence are concerned which are not derived directly from experience." (De Morgan 1849, p.183)

A mathematical example from the De Morgan paper is summarized here because it illustrates De Morgan's point above that the value of a divergent series was dependent upon the context in which it arose. This particular example contrasted the different value assigned to the same divergent series depending on whether it arose in the context of integration or when it arose algebraically. In modern terminology arising algebraically meant arising in the context of analytic continuation. There are similar examples in the Burkhardt paper from the early nineteenth century (Burkhardt 1911, p.1-3).

Consider the integral

$$I = \int_0^\infty 2^x \; dx$$

as a quadrature. The integral is infinite (the area under the curve is unbounded) and equal to the the following sum of integrals:

$$I = \int_0^1 2^x \; dx + \int_1^2 2^x \; dx + \int_2^3 2^x \; dx + \cdots$$

which when integrated is

$$\frac{1+2+4+8+\cdots}{\log(2)}$$

with the conclusion that

$$1+2+4+8+\cdots$$

is infinite by comparison with the value of the improper integral I.

On the other hand, De Morgan noted, it is often concluded that

$$1+2+4+8+\cdots=-1$$

by using the formula for the sum of a geometric series, $\frac{1}{1-x}=\sum_{n=0}^{\infty}x^n$, outside of its interval of convergence of $(-1,1)$, with the value of x equal to two.

In this case De Morgan examined the same series, $1+2+4+8+\cdots$, in two different ways. The series is divergent but depending on the method of examination, he came up with two different possibilities for the sum: -1 or ∞. The value of this divergent series depended on how it was developed. When it was developed through integration, or arithmetically, the value was infinite but if it was developed algebraically the value was -1.

That only these two results were possible was important to how De Morgan thought of divergent series as a whole. De Morgan said that, should the result of the above example come out to anything other than -1 or ∞, then divergent series should be abandoned or at least their use severely curtailed (De Morgan 1849, p.187). It is, however, possible to develop the series $1+2+4+\cdots$ via a method that does not yield either -1 or ∞. One method is to use an infinite series that is not a power series.

Consider, for example, the following three developments (Hardy 1949, p.15-16) which result in $1+2+4+8+\cdots$:

1. $(1-2x)^{-1}=1+2x+4x^2+8x^3+\cdots$, an algebraic development which gives $1+2+4+\cdots=-1$ by evaluating at $x=1$.
2. $\frac{2}{e^{2y}-1}=\frac{2}{e^{2y}+1}+\frac{4}{e^{4y}+1}+\frac{8}{e^{8y}+1}+\cdots$, another algebraic development which gives $1+2+4+\cdots=\infty$ by evaluating at $y=0$.
3. $0=x+(3x^2-x)+(7x^4-3x^2)+(15x^8-7x^4)+\cdots$, which converges to zero for $0\leq x<1$, and is a third algebraic development which gives $1+2+4+\cdots=0$ by evaluating at $x=1$.

Here we have three different algebraic developments from which three different values are obtained.

It appears that the idea of using an infinite series that was not a power series to algebraically develop $1 + 2x + 4x^2 + 8x^3 + \cdots$ and get a value of something other than -1 or ∞ did not occur to De Morgan, even though he, in the same paper, noted that $1 - 1 + 1 - 1 + \cdots$ was "a remarkable, specific case of both algebraical and trigonometrical series" (De Morgan 1849, p.184). He stated his position as follows:

> "Let $1 + 2 + 4 + \cdots$ be shown to be any thing but a root of either $1 + 2z = z$, or of another equation which has degenerated into $1 + 2z = z$; that is, let it come out any thing but -1 or ∞, and as a result of any process which does not involve integration performed on a divergent series — and I shall then be obliged to confess that divergent series must be abandoned, or rather, that the generalizations frequently made on the subject must be much curtailed. But nevertheless, there is nothing to lead us to doubt that divergent series of all classes, whether of finite or infinite divergence, must be treated alike." (De Morgan 1849, p.187)

This emphasis on integration as the only arithmetic process is something that, on reflection during the mid-twentieth century, struck Hardy as unusual. Hardy said:

> "The emphasis on integration is odd, but de Morgan seems to have regarded integration as an 'essentially arithmetic' process liable to destroy any more 'symbolic' reasoning." (Hardy 1949, p.19)

De Morgan quoted, in the original French, from a 1823 paper of Poisson (Poisson 1823) to show that Poisson firmly rejected the use of divergent series. Poisson said:

> "On enseigne dans les élémens, qu'une série divergente ne peut servir à calculer la valeur approchée de la fonction dont elle résulte par le développement: mais *quelquefois* on a paru croire qu'une telle série peut être employée dans les calculs analytiques à la place de la fonction; et *quoique cette erreur soit loin d'être générale parmi les géomètres*, il n'est cependant pas inutile de la signaler, car les résultats auxquels on parvient par l'intermédiaire des séries divergentes, *sont toujours incertains et le plus souvent inexacts*."[2] (De Morgan 1849, p.183)

[2]'We teach in the elements, that a divergent series cannot be used to compute the approximate value of the function from which it results by expansion: but sometimes it has been considered that such a series can

where the emphasis was added by De Morgan. Note the eighteenth century idea, used by Poisson, that an infinite series was the result of a function expansion. As we shall see, this contrasts with Stokes, who sometimes saw infinite series as arising naturally from the analysis of physical phenomena.

De Morgan claimed that Poisson was working on questions of mathematical physics where he substituted definite integrals for series in his work. In this context of integration, De Morgan felt Poisson was fully justified in wanting to reject the use of infinite divergent series because the value for the divergent series was not being obtained as the limit of a convergent series. Poisson was, on the other hand, accepting of finite diverging series, that is series whose partial sums to not approach a limit but remain bounded, which can be seen as a limiting form of convergence. This, in effect, appears to mean that it is possible to use an algebraically equivalent expression to evaluate a series for all of the values of the variable for which the series is convergent and also for the first value at which it is not, but not for any values beyond that. This is equivalent to using a series that converges on an open interval at the endpoint of the interval — this is Abel's theorem. In modern terms, Abel's theorem can be stated as follows. Let

$$F(x) = \sum_{k=0}^{\infty} a_k x^k$$

be a power series with real coefficients and radius of convergence of one and suppose that $\sum_{k=0}^{\infty} a_k$ converges. Then

$$\lim_{x \to 1^-} F(x) = \sum_{k=0}^{\infty} a_k$$

meaning that $F(x)$ is continuous from the left at $x = 1$.

So, for example, $1-1+1-1+\cdots$ can be thought of as $1-g+g^2-g^3+\cdots$ where $g = 1-\epsilon$ and ϵ is infinitely small and positive, such that g^n becomes infinitely close to one when n is large. This justifies the value of $\frac{1}{2}$ for $1-1+1-1+\cdots$ because "the departure from finite divergence, and commencement of real convergence, is infinitely distant" (De Morgan 1849, p.184).

Poisson was willing to "walk on the line which separates convergency from divergency, but not cross that line, even by an infinitely small quantity" (De Morgan 1849, p.184). De

be used in analytical calculations instead of the function; and although this error is far from being general among geometers, it is nevertheless not useless to point it out, because the results which one arrives by the intermediary of the divergent series, are always uncertain and most often inaccurate.".

Morgan claimed that this was equivalent to adopting the rule that "whatever is true up to the limit is true at the limit" (De Morgan 1849, p.185) but not beyond.

De Morgan felt that not just Poisson but also Fourier and Cauchy signified that $1 - 1 + 1 - 1 + \cdots = \frac{1}{2}$. This was not a trivially picked example used in order to discuss the difference between arithmetically equal and algebraically equal — this type of issue was embedded in the fabric of periodic series and integrals and, claimed De Morgan, the theory of periodic series would fail if it were possible that $1-1+1-1+\cdots$ had a particular value when considered as a limiting value of one convergent series and had a different value when considered as the limiting value of a different convergent series.

It is, however, possible to find a function that does not evaluate to $\frac{1}{2}$ but which, when expanded in a series, produces $1 - 1 + 1 - 1 + \cdots$. For example, for any $m < n$, consider

$$\frac{1 + x + \cdots + x^{m-1}}{1 + x + \cdots + x^{n-1}} = \frac{1 - x^m}{1 - x^n} = 1 - x^m + x^n - x^{n+m} + x^{2n} - \cdots$$

which gives the sum m/n for $1-1+1-1+\cdots$ (Hardy 1949, p.14). There was good reason for De Morgan to emphasize the context in which divergent series arose and he was correct in emphasizing the difficulty with integration.

De Morgan thus came to the conclusion that infinite series should not be used without consideration of the particular function that generated them. To make this distinction, De Morgan introduced the word *involopment*, by which he meant the function from which you can algebraically generate the infinite series, even though you may not be able to arithmetically use the involopment to evaluate the function for all values of x. The meaning and use of this term was explained by John Radford Young (1799-1885) in *A course of Elementary Mathematics* (Young 1862), where Young claimed that this word was introduced by De Morgan. Young said:

> "By involopment (a term very judiciously introduced by De Morgan) we mean the fraction which generates the series, and whatever besides may be included in the "&c.". In all interminable algebraic series, the "&c." stands for the involopment, *minus* the series itself: when the involopment is, as above, an algebraic fraction, the "&c." represents the remainder with the divisor underneath. We have not, therefore, as is customary, called this involopment the *sum* of the series to infinity, inasmuch as it is this and something more." (Young 1862, p.166)

Using his term involopment, De Morgan said:

> "I do not dispute that the arithmetical value of a specific case of a series may, when the particular case is convergent, be calculated: but, speaking of general series, it seems to me that it is dangerous to reason upon them until as general an involvement is found; after which, I incline to think that all conclusions upon the series should be upon them considered as the developments of those particular functions which produced them." (De Morgan 1849, p.186)

There were several specific reasons why De Morgan wanted to limit the use of divergent series to series whose underlying functions were known and they were:

1. Points of discontinuity of a function cannot be found from the series if you don't have the function from which the series was developed. This is not true and Stokes addressed this in 1847.

2. Cases of actual infinity are not distinguishable from cases of infinity resulting from developed forms of a finite quantity.

3. Infinitely divergent series may appear as very different things in different cases.

De Morgan stated this conclusion in the following way:

> "My conclusion is, that a divergent series may have for its proper value either that which is usually so considered, or infinity, according to the nature of the function from which it is expanded." (De Morgan 1849, p.187)

For De Morgan this implied that an infinite divergent series, which had a value determined from its underlying algebra to be -1, could have only two possible values: -1 or ∞. De Morgan stated that divergent series were to be abandoned if any cases were found that violated this rule that were not the result of integration of a divergent series.

By way of example, consider again the series

$$1 + 2 + 4 + 8 + \cdots$$

and then note that

$$1 + 2 + 4 + \cdots = 1 + 2(1 + 2 + 4 + \cdots)$$

such that

$$z = 1 + 2 + 4 + \cdots$$

is a solution of

$$z = 1 + 2z$$

The other solution of this equation is -1. Since $1+2+4+\cdots$ is infinite, $z = 1+2z$ has two solutions, -1 and ∞, and the sum $1+2+4+\cdots$ has two possible values: -1, ∞ and no others. There are no other solutions because $1+2+4+\cdots$ is being considered in relation to the algebraic expression $z = 1+2z$.

After analyzing infinitely divergent series as distinct from finitely divergent series, De Morgan considered dividing divergent series into two separate types in a different way — the ones that have all positive terms and the ones that do not; either because they alternate or because they have parcels of terms that are alternately positive and negative.

By defining a definite integral of a continuous function as a limit of a sum, De Morgan considered integration to be an arithmetic operation and thus the integral of the function was guaranteed to exist even if it was not possible to find an anti-derivative except in terms of an infinite series. This definition was arithmetical, not algebraic.

De Morgan continued this section of the paper with a series of examples to show that integration of a divergent series required caution — integration must not be employed unreservedly. Further, definite integrals, evaluated at different values of a parameter, can and do experience discontinuities in output for some values of the parameter. And, according to De Morgan, the integral and the series should produce the mean of those two values at that point.

The De Morgan paper continued with the claim that alternating divergent series stood on safer basis than those whose terms are all of the same sign, noting that the error in a convergent alternating series is bounded by the first neglected term and further that this had been *observed* (emphasis is in the original) to be the case for divergent series as well. It was this fact that made an alternating divergent series as useful as a convergent series, in practice.

Another point in this paper worth remarking on is that De Morgan noted that trigonometrical series of the "most continuous form have been shown to represent functions of the most capricious discontinuity" (De Morgan 1849, p.198). This again implied, for De Morgan, that caution was required when performing an inverse operation (e.g. that of integration). An example of a function, discontinuous at $x = (2n+1)\pi, n \in \mathbb{Z}$, is the sawtooth function

$$\begin{cases} s(x) = \dfrac{x}{\pi} & -\pi < x < \pi \\ s(x+2\pi k) = s(x) & -\pi < x < \pi \text{ and } k \in \mathbb{Z} \end{cases}$$

which has a Fourier series of

$$s(x) = \frac{2}{\pi} \sum_{n=1}^{\infty} \frac{(-1)^{n+1}}{n} \sin(nx).$$

which converges to 0 for values of x which are odd multiples of π. For De Morgan, Fourier series like this example appeared to be continuous for all x and the points of discontinuity of the underlying function were not determinable from the Fourier series representation. Further, the convergence to the mean of the function at the left and right endpoint of the function discontinuity, which is what De Morgan felt should happen, was not assured. The tools to properly analyze these types of issues were not available in 1844 and De Morgan's statements reflect the confusion and concern about that and lent authority to his statement that caution was required when using integration.

De Morgan published this account in 1844 and, in the main, he reflected on the work of Poisson, some of which he agreed with and some of which he tried to overturn. Stokes would certainly have read De Morgan's paper, likely before writing his paper of 1847 which will be analyzed in Chapter 3 of this thesis. I did not find any correspondence between Stokes and De Morgan in the collected correspondence of Stokes but given that the De Morgan's work was published in the *Transactions of the Cambridge Philosophical Society*, it is reasonable to conclude that Stokes read the 1844 paper of De Morgan.

There are two further examples of the use of divergent series prior to 1847 during the time when their use was contested and not well understood that I examine because they demonstrate themes that will echo later in the thesis. And finally, I examine an additional example of the use of divergent series from 1868 which demonstrates some of the issues raised by De Morgan.

1.1.4 Liouville, Green, and Asymptotic Expansions in 1837

As stated earlier in this introduction, I argue that it was the work of Stokes, Poincaré and Stieltjes that was instrumental in the development of asymptotic expansions. And, as we have just seen, the state of the mathematics of divergent series at mid-nineteenth century was well-documented at that time. There are however a few episodes where asymptotic series solutions were obtained prior to 1850 that bear mentioning.

For example, both Joseph Liouville (1809-1882) and George Green (1793-1841), in 1837, used a method identified by Kline (Kline 1990, p.1101) to find an asymptotic solution. These deserve special mention since, in both cases, the first term of an asymptotic series solution to a differential equation was found. Independently of one another, both Liouville and Green found an approximate solution to different differential equations, an approximation which, unknown to them, was asymptotic. Green's work resulted in a solution to a particular physical problem whereas the work of Liouville was of a strictly mathematical nature.

Green published his approximation in a paper titled *On the Motion of Waves in a Variable Canal of Small Depth and Width* (Green 1838, p.457-62) where he used a method of Lagrange to develop a differential equation to model the motion of fluid in a canal with restricted dimension. Green was able to give an approximate solution to this differential equation. He used a substitution into the differential equation which transformed the differential equation into an integral equation. This resulting integral equation was solved by a method similar to that of solution by successive approximation.

Liouville published his work in a paper titled *Second Mémoire sur le développement des fonctions ou parties de fonctions en séries dont les divers termes sont assujétis à satisfaire à une même équation différentielle du second ordre, contenant un paramètre variable* (Liouville 1837). In this paper Liouville, using the same method as Green, found approximate solutions to the differential equation

$$\frac{d}{dx}\left(p\frac{dy}{dx}\right) + \left(\lambda^2 q_0 + q_1\right) y = 0$$

where p, q_0, and q_1 are positive functions of x and λ is a parameter. The solution Liouvelle found was valid for large values of λ. Neither Green nor Liouville discussed the conditions under which the solution was valid. No error term or bound on the error of the approximation found was given either.

Prefiguring what we will see in this thesis to be the different character between the work of the French and English researchers developing asymptotic expansions during the second half of the nineteenth century, the British work of Green was firmly embedded in the solution of a specific physical problem, and the French work of Liouville was of a more general, purely mathematical, nature.

In Jesper Lützen's scientific biography of Liouville (Lützen 1990), in the chapter titled *Liouville's Mature Papers on Second-Order Differential Equations*, Lützen showed how Liouville rigorously established the asymptotic behaviour of the eigenvalues he found. Lützen further footnoted that both Kline (Kline 1990) and Schlissel (Schlissel 1977) counted this as one of the earliest uses of asymptotic series.

Schlissel, in his paper *The Development of Asymptotic Solutions of Linear Ordinary Differential Equations, 1817-1920* (Schlissel 1977), highlighted several appearances of asymptotic expansions during the early nineteenth century beyond those identified by Kline.

Schlissel claimed that the earliest appearance of an approximate solution in decreasing powers of the variable was in the 1817 work of Francesco Carlini (1783-1849) whose work involved investigating the elliptic motion of a planet about the sun. Even though Carlini's result was replicated in the late nineteenth century by more rigorous means, I do not think that this result should be considered an asymptotic expansion since Carlini did not recognize it as such. As Schlissel stated:

> "Carlini gave no indication of the sense in which the term
>
> $$\omega(x) \approx \frac{x^n \exp\left\{n\sqrt{1-x^2}\right\}}{\sqrt{\frac{\pi}{2}}n^{\frac{3}{2}}\left(1-x^2\right)^{\frac{1}{4}}\left[1+\sqrt{1-x^2}\right]^n}$$
>
> approximates an actual solution of
>
> $$x^2\frac{d^2w}{dx^2} + x\frac{dw}{dx} - n^2\left(1-x^2\right)w = 0$$
>
> and his method does not lend itself to any error estimate." (Schlissel 1977, p.310)

There appears to be no influence from the work of Carlini, Green or Liouville on Stokes, Poincaré or Stieltjes, and neither Carlini, Green nor Liouville were aware that they had provided a very specific type of approximate solution. Further, the approximations these mathematicians provided were only one or two terms rather than a complete formal series solution. These early examples of asymptotic analysis will not be further discussed in this thesis.

Schlissel summarized this early episode in the following way:

> "Each investigator in this preliminary stage obtained an expression which he claimed was an approximation to the actual solution, but none attempted to estimate the error. These three papers, though their results were modest and their methods questionable, contained the seeds of many of the methods used by later investigators." (Schlissel 1977, p.314)

A few basic results were obtained in this early research but the use of rigorous asymptotic expansions with error bounds was still to come.

1.1.5 Hankel in 1868

Some of the issues raised by De Morgan surfaced elsewhere. An example is afforded by Hankel's idea of semi-convergent series. Hermann Hankel (1839-1873) in 1868 paper, titled

Die Cylinderfunctionen erster und zweiter Art (Hankel 1869), examined approximations to the Bessel functions.

Again, this work does not appear to have had any influence on Stieltjes or Poincaré, and it was published after Stokes' work. Here I simply note, following Schlissel, that Hankel obtained series approximations for what are now called the Hankel functions. These are $\mathrm{H}_\alpha^{(1)} = \mathrm{J}_\alpha + i\mathrm{Y}_\alpha$ and $\mathrm{H}_\alpha^{(2)} = \mathrm{J}_\alpha - i\mathrm{Y}_\alpha$ where J_α and Y_α are the Bessel functions of the first and second kind of order α.

Hankel called the approximations he found semi-convergent — a term that Stieltjes will later use to mean something different. Hankel defined semi-convergent to mean the following:

> "The series $S(x) = \displaystyle\sum_{j=0}^{\infty} a_j(x)$ is said to be semi-convergent to a function $f(x)$ for some x interval, if for any integer N, $\left| f(x) - \displaystyle\sum_{j=0}^{N} a_j(x) \right| < |a_{N+1}(x)|$." (Schlissel 1977, p.319)

Given that this definition means that a series is semi-convergent precisely when the difference between the function and the sum of the first n terms of the series is less in absolute value than the $(n+1)^{th}$ term, the error in using the series $S(x)$ was bounded by the first term omitted and as such this definition only applies to alternating series. Further, Hankel's analysis used the property that this semi-convergent property was preserved under integration. He also noted what came to be called the Stokes phenomenon, as we discuss later, that appeared when the argument of the variable crossed the negative y-axis.

Chapter 2

Precision, the Pendulum and Stokes

2.1 The Desire for Precision

In order to help understand the milieu in which the use of asymptotic approximations to divergent series arose in Britain, I now consider the topic of precision. The role of careful, accurate measurement in the advancement of science began during the late eighteenth century and became increasingly important in the first half of the nineteenth century. Indeed, in the subsequent chapters, I will show that Stokes' use of divergent series was motivated by a desire for precision in optical and astronomical computations.

Instruments, Travel and Science: Itineraries of Precision form [sic] the Seventeenth to the Twentieth Century, edited by Marie-Noëlle Bourguet, Christian Licoppe, and H. Otto Sibum, is a collection of articles which "investigates the historical development of the underlying relationship between instruments, travel and natural knowledge which gave rise to modern science" (Bourguet, Licoppe & Sibum 2002, p.ii). The authors collectively claim that from the mid-seventeenth century onwards, natural knowledge came about increasingly from measurement rather than from the mind of a philosopher with the result that, by 1799, "data were then about to become the stuff of the sciences and instrumental procedures the path towards scientific achievement" (Bourguet *et al.* 2002, p.3).

The gathering of data, via instruments, allowed for a description of nature that was uniform and regular. It made possible the movement of data and comparison of the understanding of the natural world between locations. As Bourguet said:

> "instruments of precision have become a privileged means of bridging the gap between heterogeneous places." (Bourguet *et al.* 2002, p.8)

The use of instruments of precision, in Britain, was part of the imperial project with data being collected for far more than natural philosophical reasons. It was perhaps key, as Schaffer stated (Schaffer 1995, p.136), to imperial power. A clear example of the relationship between imperialism and precision is afforded by the Great Trigonometrical Survey of India. This survey was an immense undertaking which ran from 1802 to 1871 with the goal of surveying the entire Indian subcontinent with scientific precision. The outcomes of the survey were both political and scientific — complete geographical information for taxation, administrative and military purposes and the first accurate measurement of a section of an arc or longitude, for example. There is voluminous literature on the history and impact of Great Trigonometrical Survey providing analysis from how the Survey informed Britain's dominion over India to the type of scientific advances that were made in order to perform the survey. See for example (Edney 1997) or (Keay 2000).

Thus the motivations for precise quantification, including those arising from the quantified description of natural phenomena, were partly a desire to mathematize nature, but there was also, perhaps more importantly, a desire to use precision in the regulation and control of the activities of society.

The Values of Precision is a collection of essays edited by M. Norton Wise (Wise 1995) in which the case is made by various authors that precision came to occupy an increasingly important place in science as well as in society in general by the latter half of the nineteenth century. In Wise's introduction to this volume, he stated that the "unproblematic" core (Wise 1995, p.4) of several of the best known theses on the increasing importance of precision is that, as the nineteenth century progressed, the desire for precision extended from optics and astronomy to a broader spectrum of activities including chemistry, electricity, magnetism, and even air quality.

The increasing importance of precision as a societal value has been written about by several authors, with differing understandings of how this came about. For example, Wise claimed that it was the practical concerns of the business world that drove the interest in quantification. Quantification, Wise stated:

> "derived from the need of administrators for reliable information about particular aspects of the world in order to be able to make reasonable plans: availability of human and material resources, cost estimates, tax revenues, life and annuity tables, maps, location of ships, etc." (Wise 1995, p.5)

Wise is claiming that the needs of operating the economy drove the quantification in that realm. It is reasonable to ask if this interest was independent of the quantification that the sciences were contemporaneously undergoing or if one may have lead to the other.

Ted Porter, in his book *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Porter 2020) argued that it is, at least, not obvious that earlier success with quantification in science led to more precision in the management of the affairs of society. He considered that, even though it may be typically assumed that the value of precision moved from science to industry and business, the interaction may have gone in the other direction or perhaps back and forth. Regardless of where precision was first shown to have great utility, Porter argued that by carefully examining the quantification of business we can learn something about the quantification of science. Porter said:

> "How are we to account for the prestige and power of quantitative methods in the modern world? The usual answer, given by apologists and critics alike, is that quantification became a desideratum of social and economic investigation as a result of its successes in the study of nature. I am not content with this answer. It is not quite empty, but it begs some crucial questions. Why should the kind of success achieved in the study of stars, molecules, or cells have come to seem an attractive model for research on human societies? And, indeed, how should we understand the near ubiquity of quantification in the sciences of nature? I intend this book to display the advantages of pointing the arrow of explanation in the opposite direction. When we begin to comprehend the overwhelming appeal of quantification in business, government, and social research, we will also have learned something new about its role in physical chemistry and ecology." (Porter 2020, p.xx)

Other authors have described the movement to precision in variety of different ways. Thomas Kuhn called it a "second scientific revolution" (Kuhn 1961, p.188) in his 1961 article *The Function of Measurement in Modern Physical Science*. John Heilbron (see for example his essay titled *A Mathematicians' Mutiny, with Morals* in (Horwich 1993)) has written on the increased importance of mathematics in the physical sciences and Ian Hacking has written about the "avalanche of numbers" (Cisney & Morar (eds.) 2016, p.65) produced during the early nineteenth century as evidence for his thesis that truth is discovered through empirical observation and induction.

The move to quantify nature was not adopted without criticism or concern. Both Goethe and Humboldt, early in the nineteenth century, had concerns that something may be lost in the rationalizing of nature. As Bourget said:

> "Investigators, particularly Goethe, then expressed their fear of a loss of authenticity in the aesthetic and emotional experience of Nature, if instruments were

> to be used in a systematic approach towards the world. However passionate about measurements and calculations, Humboldt himself sometimes feared that a quantified science would drive humanity away from a holistic experience of Nature and cause an impoverishment in one's sense of self." (Bourguet *et al.* 2002, p.12)

The following quote from Goethe clarifies and reinforces the sentiment captured above that the move to quantification was not seen as entirely positive. Goethe, as quoted by Wood, said:

> "In broad daylight inscrutable, Nature does not suffer her veil to be taken from her, and what she does not reveal to the spirit, thou wilt not wrest from her by levers and screws." (Wood 1893, p.119)

This type of concern about the use of empirical observation and precise measurement in the scientific process mostly disappeared over the course of the nineteenth century. A quote by British mathematical physicist William Thomson, 1^{st} Baron Kelvin (1824-1907) from 1883 emphatically stated that, by this time, science was about numbers and measurement. Kelvin said, as quoted in Bourget:

> "I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely advanced to the stage of science, whatever the matter may be." (Bourguet *et al.* 2002, p.16)

Kelvin was a very close colleague of Stokes' and the above quote is taken from a popular lecture on electrical units of measurement which Kelvin gave to the Institute of Civil Engineers. It is again a reflection of the importance of precise standards.

As we shall see shortly, Sir George Biddell Airy (1801-1892), a mathematical physicist, communicated precise optical observations to Stokes — observations which Stokes then used to verify calculations. Airy is a good example of a nineteenth century scientist whose drive for precision went well beyond science, and he is illustrative of the value placed on precision in science as well as in society.

For Airy, precision and quantification began early in his life with the record keeping of his personal finances and then it extended into his scientific career. Airy's autobiography begins with an introduction written by his son in which the son claimed that for Airy, order, record keeping, and precision were vitally important. Airy's son stated that:

> “The ruling feature of his [Airy’s] character was undoubtedly Order. From the time that he went up to Cambridge to the end of his life his system of order was strictly maintained. [...] His accounts were perfectly kept by double entry throughout his life, and he valued extremely the order of book-keeping: this facility of keeping accounts was very useful to him. [...] To a high appreciation of order he attributed in a great degree his command of mathematics, and sometimes spoke of mathematics as nothing more than a system of order carried to a considerable extent. In everything he was methodical and orderly, and he had the greatest dread of disorder creeping into the routine work of the Observatory, even in the smallest matters.” (Airy & Airy 1896, p.2)

This observation about the life and habits of a major figure (Airy was knighted on June 18, 1872) of nineteenth century British mathematical physics speaks to precision both in science and in society and is an example of what Porter claimed — that it is instructive to observe that quantification in one area led to it in another.

Stokes’ work in the decade preceding 1850 was mathematical and the motivation for developing mathematical laws of nature was for “rationalizing design of instruments, establishing theoretical expectations for response, and revealing sources of error” (Wise 1995, p.5). The precision measurements or computations he used often came from people who, by way of their occupation or interests, were interested in precision for reasons of commerce, industry or social organization. For example, as mentioned previously, Stokes used the precise pendulum measurements of Miller in his discovery of the index of friction. Miller, however, produced those measurements as part of project to use the pendulum as a method to recapture the exact length of a yard should that standard ever be lost as indeed it was, during a fire in 1834. The measurements were made for one reason and then a mathematical physicist used the measurements for an entirely different reason.

In the two or three decades following 1850, computed and measured numbers were being used to determine or to suggest underlying physical structure. As is detailed later in this chapter, physical discovery came about, in the work of Stokes, as the result of the comparison of precise laboratory measurements with those predicted (using asymptotic expansions among other methods) from theoretical understanding. It is also the case that comparing precision measurements with what was expected from competing underlying physical theories sometimes permitted a decision to be made between those theories. It was, of course, not always the case that the measurement allowed this determination to be made — for example, it was possible that competing theories could produce the same predicted outcome.

A very clear example of measurement helping to select between competing physical theories occurred during the resolution to the question of what light is. By 1850, the conceptualization of light had changed from it being a particle to it being a type of wave. What the wave consisted of or how it propagated was at that point unclear.

James Clerk Maxwell (1831-1879) had a model of a mechanical ether (at the time the substance that was widely thought to fill space) that allowed him to predict the velocity of the propagation of electromagnetic waves in an ether-filled space. The velocity of those waves was dependent on the elasticity and density of the ether. In a vacuum, by contrast, the electromagnetic wave velocity could be calculated from the ratio between two units of charge, the electrostatic and the electromagnetic. Maxwell's model predicted that the the ratio between the electrostatic and the electromagnetic units, a number denoted by ν, would be equal to the speed of light, denoted c. The ratio, ν, was measured in Göttingen in the late 1850s. The speed of light was measured independently in Paris in the 1840s.

These two numbers were found, by Wilhelm Eduard Weber (1804-1891) and Rudolf Kohlrausch (1809-1858) in 1855 (Kirchner 1957), to be within 1% of each other, but, given potential measurement errors of c as well as simplifications in the model of the ether which affected the calculation of ν, it remained controversial as to whether $c = \nu$ or not. The fundamental understanding of what light was rested on this decision of whether $c = \nu$. Was light a transverse electromagnetic wave or a longitudinal mechanical motion through a rigid elastic ether? Maxwell concluded from the close agreement between c and ν that light was transverse waves in an electromagnetic ether (Schaffer 1995, p.137).

It took three decades for it to be agreed upon that the value of ν was in fact equal to the value of the speed of light. This example illustrates how precise measurement was used to answer the question: Is light the same thing as electromagnetic transmission of telegraph signals? Showing that the speed of the these two things were the same with an extraordinarily low amount of error was a proxy for showing that they were the same phenomenon. Thus it was extraordinarily important that those measurements were as accurate as possible.

In the essay, *Accurate Measurement is an English Science* (Schaffer 1995, p.135), Schaffer argued that, during the mid-nineteenth century, the focus on precision was a particularly British preoccupation, and that precise metrology was vital for commercial and military superiority, partially because one key to imperial power was that what is done and works well in one location can then be made to do so in another location. Schaffer uses a variety of examples throughout his essay to advance his argument.

One of Schaffer's examples was the 1870s visit to Europe of American Henry Rowland, a founding faculty member at John Hopkins University. He arrived with a sizeable budget to purchase precision laboratory instruments and he visited England (including Maxwell's country estate at Glenlair in Scotland), Germany, and Austria to observe the instrumentation used in many laboratories. Rowland admired the British for their use of instrumentation, both for making physical phenomena less vague and also for precision measurement. The two quotes below capture Rowland's observations of national differences in the use of apparatus in the second half of the nineteenth century — in this case regarding apparatus used in the determination of the unit of electrical resistance. Rowland said:

> "While in Göttingen, I had the pleasure of seeing the apparatus used by Gauss and Weber and also that more recently used by Kohlrausch in the determination of the absolute value of Siemens' unit... So far it seems to me that the accurate measurement of resistance either absolutely or relatively is an English science almost unknown in Germany." (Schaffer 1995, p.139)

> "In America we have apparatus for illustration, in England and France they have apparatus for illustration and experiment but in Germany they have only apparatus for experimental investigation." (Schaffer 1995, p.143)

According to Schaffer, Rowland's thoughts on the requirement of quality instruments for proper experimentation and for illustration were influenced by Maxwell, which Schaffer supported by using several quotes taken from the reports that Rowland wrote about his European journey. Maxwell's view was that a period of increasingly accurate measurements predates advances in the theoretical understanding of the world. In his 1871 inaugural lecture at Cambridge, Maxwell said:

> "The history of science shows that even during that phase of her progress in which she devotes herself to improving the accuracy of numerical measurement of quantities with which she has long been familiar, she is preparing the material for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers." (Schaffer 1995, p.144)

This is a clear statement that the time spent in the making and using of precision measurement instruments was a vital component of scientific progress. Illustration instruments were, on the one hand, pedagogical. On the other hand, they were scientific proofs of concept in a kind of engineering sense, at least for Maxwell and Kelvin. For example, Maxwell made a device that was intended to mimic the motion of particles in the rings of Saturn, and Kelvin had a tidal simulator.

H. Otto Sibum's essay titled *Exploring the Margins of Precision* in (Bourguet *et al.* 2002), analysed Rowland's visit and his observations of European science and concluded that Rowland was impressed by this emerging empirical culture. Sibum himself concluded that:

> "Absolute measures were true representations of nature. Accuracy thereby became the basic virtue of a new, evermore fully established exact science, which also accorded well with the values of Victorian England." (Bourguet *et al.* 2002, p.219)

Again the conclusion is that precision was a dominating characteristic of this time. It is seen contemporaneously as being important in science, in business, in the imperial ambitions of Britain and as an important moral characteristic of Victorian England.

During this same time period (the 1850s) there are other celebrated examples of precision measurements being used in a large variety of ways. The accurate determination of the speed of light is one important example. The measurement of the position of the dark bands in the envelope of light (called a caustic) that results when light is refracted or reflected by a curved surface of an object is another example. Moreover, precise pendulum computations and measurements enabled accurate determination of the gravitational constant which, in turn, allowed a determination of the shape of the earth.

The example of the precise determination of the speed of light illustrates what Warwick (Warwick 1995, p.312) claimed was the interest of most British mathematical physicists of the nineteenth century — they were more concerned with predicting what they experimentally observed than they were concerned about developing theory as a tool for understanding the universe. Warwick said it thus:

> "[they were] not concerned primarily with the invention of grand new theories, but with the much more workaday problems of adapting extant theoretical tools to new applications. It is, moreover, the general invisibility of this important work that gives rise to the sense that fundamental theories make precise and unequivocal predictions across a wide range of physical phenomena. ... In practice the range of skills and artifices required to develop useful mathematical models of physical phenomena are as important to the physicist as are the fundamental laws themselves." (Warwick 1995, p.312)

As we shall see in Chapter 3, Stokes used precision measurement to validate physical theory as well as to demonstrate the correctness of his mathematical methods. He was thus part of a greater movement of his time and he was informed about the recent elucidations of physical theory via the use of measurement. French physicist Léon Foucault (1818-1868) communicated with Stokes in May of 1857 about the determination of the speed of light and in 1854 Thomson told Stokes about the speed of transmission of telegraph signals which also determined ν (Thomson 1854).

Warwick's analysis is confirmed by what we will see in the behaviour of Stokes. Stokes used divergent series to get results from theory and then verify them against experimental results. Not only is this an example of a computational procedure, it is an example of a computational procedure that had a history in the period leading up to Stokes' use of it as something that was of unknown validity and, in fact, thought by some to be something which should be mostly banned from mathematics as was detailed in the introduction.

However, whether numerical agreement between experiment and theory verifies theory is itself subject to differing positions. For example, Thomson was not convinced that, given the one percent difference between the values of ν and c, that light and telegraph signals were the same phenomenon (Schaffer 1995, p.137-138).

Warwick introduced the philosophy of Nancy Cartwright (1944-) into his discussion because she discusses how and when experimental results can be used to make decisions — exactly the source of Thomson's unease. Cartwright is an American philosopher of science whose pragmatic philosophy concerns itself with how science achieves success. Her claim was that the correspondence between an experimentally determined value and its equivalent theoretically generated numerical value cannot be used to validate theory. Cartwright claimed that:

> "these correspondences cannot be said to provide direct evidence for the the truth of the fundamental laws themselves because the process of prediction requires the adoption of approximations that are not dictated by laws." (Warwick 1995, p.312).

Warwick used this type of argument to further his thesis that computational procedures form a crucial interface between mathematical theory and experiment. Therefore they must be included in any discussion of the role of precision in the historical development of the exact sciences (Warwick 1995, p. 313).

In contrast, we shall see that Stokes simply does not express himself on the kind of issues brought up by Cartwright and Warwick. Stokes does not convey any concern about how he got his numerical results from his theory and he was completely satisfied as to their utility after he obtained agreement with experimental values either from Francis Baily's (1774-1844) pendulum measurements or, as I discuss below, from Airy's optical measurements. Further, Stokes used, when possible, the numerical agreement of his results with results obtained by other computational procedures to justify the validity of his computational procedures.

During the 1840s and 1850s, a period of time in which there was much consternation and discussion about the use of divergent series, Stokes simply chose to use them and got experimentally verifiable numbers. Did this pragmatic use of divergent series by Stokes, a major figure in mathematical physics in Britain, hasten or make acceptable these methods and cause research into divergent series to be accelerated?

Warwick pointed to the importance of paying attention to how calculations are done, because computation is a human activity whose reliability is dependent on the method and technology used. Computational methods can be simple, complex, laborious, sophisticated, uncommon, widespread or require specialized equipment. Stokes' numerical calculations replaced simple but laborious calculations with efficient calculations, but those calculations required substantial expertise and had uncertain validity. Warwick said:

> "When the methods and uses of calculation change, we should look for an historical explanation of why that change is taking place and what resources are enabling it to occur." (Warwick 2003, p.317)

Stokes used precise pendulum measurements to verify new theory and those measurements were available to him because precision was seen as important to society. I illustrate this in the next section in the context of the pendulum measurements made by Baily.

2.2 The Importance of the Pendulum

2.2.1 Francis Baily and the Empirical Pendulum

Baily started his very successful career in business working at the stock exchange. He wrote about and produced tables for the pricing of leases and annuities which provided efficiency, accuracy and precision in making life-contingency decisions. By 1825 he was independently wealthy, after which he retired and spent the remainder of his time on investigations primarily related to astronomy.

Baily had many interests as can be seen in the diversity of the topics of the ninety papers that he published. The topics include leasing, interest and annuities, life insurance, history, astronomy (solar eclipses, shape of the earth), navigation and pendulums. While these topics may appear to be somewhat disparate, for Baily they were tied together by a desire to present clear, accurate information on which good decisions could be made.

Baily believed that tables of accurate computations were important. He asserted in an 1824 paper titled *On Mr. Babbage's New Machine for Calculating and Printing Mathematical and Astronomical Tables* (Babbage 1889, p.225) that:

> "The great object of all tables is to save time and labour, and to prevent the occurrence of error in various computations." (Babbage 1889, p.226)

and further that accurate tables of calculations were in service of the promotion of science. Baily said:

> "The substitution, however, of the unvarying action of machinery [this is the use of Babbage's mechanical calculator] for this laborious yet uncertain operation of the mind, confers an extent of practical power and utility on the method of differences, unrivalled by anything which it has hitherto produced: and which will in various ways tend to the promotion of science." (Babbage 1889, p.226)

This sentiment was true not just for tables of interest and annuities but also for astronomy. The extensive pendulum measurements that Baily made built upon the work of the British naval officer Henry Foster (1797-1831) and were partly in service to astronomy.

Pendulum measurements were vital in the preparation of tables for navigation. In response to a lack in existing nautical tables, Baily, John Herschel (1792-1871) and Charles Babbage (1791-1871) successfully founded, during the early 1820s, the Astronomical Society of London in order to support astronomical research. At the time of founding, most members of the society were 'gentleman astronomers' rather than professionals. Baily, in fact, was not exceptional in his interests outside of astronomy — many early members of the Astronomical Society were actuaries or businessmen (Ashworth 1994, p.410).

The Astronomical Society of London became the Royal Astronomical Society in 1831 upon receiving a Royal Charter from William IV. The society sought to clearly define the boundaries of the science of astronomy and "to remove astronomical speculation and place it on a solid calculating base" (Ashworth 1994, p.412).

Baily was concerned that British science was falling behind; that, for example, the Nautical Almanac prepared by the Board of Longitude, seen as vital for British naval superiority, was not being updated or corrected as advances were being made in astronomy. This concern for British science also motivated other scientific initiatives of the early nineteenth century.

For example, John Herschel, Charles Babbage, and George Peacock had earlier formed the Cambridge Analytical Society, which sought to introduce Leibnizian notation for differential calculus into Cambridge mathematical instruction. This was a measure aimed at correcting a lack of ability to make calculations efficiently using the more cumbersome Newtonian fluxion notation and to bring advances in analysis from the continent to Britain. Babbage, in the preface to the *Memoirs of the Analytical Society* (Babbage & Herschel 1813), stated it thus:

> "Discovered by Fermat, concinnated and rendered analytical by Newton, and enriched by Leibnitz with a powerful and comprehensive notation, it was presently seen that the new calculus might aspire to the loftiest ends. But, as if the soil of this country were unfavourable to its cultivation, it soon drooped and almost faded into neglect, and we have now to re-import the exotic, with nearly a century of foreign improvement, and to render it once more indigenous among us." (Babbage & Herschel 1813, p.iv)

Astronomical measurements were not only necessary for both military and commercial navigation but were also used in support of theology (Ashworth 1994, p.4). There were concerns about whether other countries were ahead on this front — for example, the Bureau des Longitudes in France. This was during the time just following the Napoleonic Wars in which Britain twice defeated Napoleon and had established itself as the world's foremost naval power.

Baily thought of himself and was perceived by others as someone who quantified things — who spared no effort to get precise measurements. In the words of Herschel, cofounder of the Astronomical Society:

> "Baily epitomized sound, thorough, precise thinking: 'everything to which he turned his thoughts', wrote John Herschel, was 'reduced to number, weight, and measure'. Baily presented himself and indeed was represented by others as the perfect citizen, reliable and uncorrupted by interests." (Ashworth 1994, p.418)

The desire for precision, as well as the use of theory as a handmaiden to that precision, Ashworth (Ashworth 1994) argued, was seen in the writing of another member of the Astronomical Society, the mathematician and actuary, Benjamin Gompertz (1779-1865). Ashworth said:

> "Gompertz wrote an extensive article on the importance of theory in correcting for an instrument's failing through calculation. He wrote, 'the instruments supply data to the theory; but it is theory which invents the instruments'. Further

it is 'theory which points out the cause of the defects; theory which directs the practitioner to the requisite improvements.' " (Ashworth 1994, p.438)

In the 1824 article, *On Mr. Babbage's new machine for calculating and printing mathematical tables* (Babbage 1889, p.225), Baily provided a history and summary of the mathematical tables produced to date. Some of the tables, like those of values of the sine or logarithm function, were for general use, but some tables were produced with a specific purpose in mind.

The values from the astronomical observation tables were further used in the production of tables for navigation. The importance of accuracy and precision was obvious here as errors in these tables could lead to difficulty or even danger for the mariners who relied on them. Baily himself found over 500 errors in the tables of the sun and the moon from which the values published in the volumes of the Nautical Almanac were computed. The Astronomical Society produced improved tables for navigation and, by end of the 1820s, the Board of Longitude was closed and the Admiralty was using tables prepared by the Astronomical Society for navigation. Baily was thus shown to be correct in his belief of the importance of up to date, accurate tables.

Thus precise measurements, and precision in general, were considered to be highly desirable in the first half of the nineteenth century. There are numerous examples of this but, in remaining portion of this chapter, I focus on how the data on pendulums and in optics that Stokes used to verify his work, both theoretical and mathematical, was acquired.

2.2.2 Physics and Empirical Validation and Verification

By the late 1840s there were at least two important quantities, both related to astronomy, which were not computable from theory but whose numerical quantities were possible to measure in the field or in the laboratory. Firstly, in the early nineteenth century, in Britain and elsewhere, the desire to determine as accurately as possible the period of the pendulum was important. This was largely because pendulum measurements were extensively used in surveying, in navigation, and in the determination of physical constants including the gravitational constant, the ellipticity of the earth, and the mean density of the earth.

Secondly, among other problems in optics, the location of more than the first few dark bands that appear in the caustic when light is shone through a curved surface was not computable from theory even though many bands were observable in the laboratory. We will return to this in the context of Stokes' work in Chapter 3.

In both of these cases, as was typical practice, very precise empirical measurements were compared to predictions made from theory. Further and specific to these two cases, it was not just that theory was being validated by comparison to measured data. In both cases there was a further mathematical difficulty because it was a non-trivial task to produce numerical values from the theory for comparison in the first place. This was due to the prohibitively large number of calculations required.

The large number of computations resulted from the necessity of evaluating definite integrals which were not integrable in finite terms. Therefore numerical results were being obtained via partial sums of convergent infinite series after integration of the power series of the integrand. However, the number of terms required to obtain the desired precision was too large for the human calculator.

Both of these problems were solved at the same time (in 1848-1850) in the work of Stokes. In this chapter I focus on questions associated with pendulums. Stokes' novel mathematical technique was developed first to solve the optical problem and then used to complete the solution of the pendulum problem. For this reason, the mathematics that Stokes developed is explained, in Chapter 3, in reference to how he used it in optics.

2.2.3 Major Pendulum Developments to 1845

2.2.3.1 The Important Theorists, Experimentalists and Explorers

There were many people working with and interested in pendulums during the first half of the nineteenth century. I have decided to divide the people into three categories: those who worked primarily on the mathematical and physical theory, those who worked primarily on obtaining precise experimental results, and those whose efforts were primarily doing field work. Naturally people often worked in more than one category.

The two most important theorists working on the pendulum project in Britain were Sir George Gabriel Stokes (1819-1903), and Sir George Biddell Airy (1801-1892). Two other important important theorists, from a bit earlier, were Friedrich Wilhelm Bessel (1784-1846), in Germany, and Siméon Denis Poisson (1781-1840), in France. These people built upon a long tradition following Galileo, Huygens and the Bernoullis.

Stokes was a towering figure in British mathematical physics and is the focal point here. He spent his entire career at Cambridge where he was the Lucasian Professor of Mathematics for an astonishing 54 years. Stokes was educated at Pembroke College, Cambridge and graduated as Senior Wrangler and first Smith's prizeman in 1841 after which he became a fellow. He resigned his fellowship in 1857 (as he was required to) upon marriage. However he was reinstated in 1869 when the rules were changed, and he maintained that fellowship until 1902.

Stokes made an enormous number of contributions to a wide variety of fields both in physics and in mathematics. In physics, Stokes made pioneering contributions to the field of fluid dynamics, a topic on which he published twenty-three papers. This work included a formulation of the Navier-Stokes equations which are used to describe fluid flow over a wide range of magnitudes. He also, of major importance, provided the theory of finite amplitude oscillatory water waves.

At the beginning of Stokes' career, there were intense discussions about light — was it corpuscular or was it a wave, and what was the effect of an assumed ether through which light moved? As a result, some of Stokes' work in fluid dynamics was also germane to optical investigations. Stokes made a multitude of contributions to optics, publishing over sixty papers that ranged from consolidating older work with sharper mathematics to extending physical optics. This included work on polarization, double refraction, optical rotation and fluorescence.

In more strictly mathematical work, he popularized Stokes' theorem in vector calculus and contributed to asymptotic expansions — this work is the main focus of this thesis. He also made contributions to engineering, primarily by enhancing the safety of railway bridges. His improvements to bridge safety came partly as a result of his mathematical analysis of bridge stability and partly as a result of participating in inquiries that were made following bridge failures.

Stokes was accorded numerous awards which include being made a baronet in 1889 and receiving the Copley medal in 1893. He was president of the Royal Society from 1885 to 1890 and was a member of the British House of Commons from 1887 to 1892. For further biographical information and an analysis and summary of Stokes' scientific work see (McCartney, Whitaker & Wood 2019).

Airy was a mathematician and astronomer who was the Astronomer Royal from 1835 to 1881. Two facets of his work are of most interest here — his work in optics which we shall see shortly and his applied use of the pendulum. Airy was in the first generation of those educated under the tutelage of the members of the Cambridge Analytical Society. Peacock was his academic advisor. He was Senior Wrangler in the Tripos examination of 1823 and first Smith's prizeman during that same year. In 1826 he wrote *Mathematical Tracts on Physical Astronomy* which linked together and covered the topics of optics, astronomy, and gravitation, naturally combining telescopes and pendulums. Also in 1826, Airy realized that measurements of the gravitational constant at the top and bottom of a mine shaft would allow him to determine the mean density of the earth, an experiment that was carried out in 1854. For biographical information and a summary of Airy's scientific work see (Airy & Airy 1896).

Bessel was a German astronomer, mathematician and physicist who is important to this thesis because of his pendulum work carried out between 1825 and 1827. Bessel was not formally trained and was granted an honorary doctorate on the recommendation of Gauss. He is perhaps best known for using stellar parallax to calculate the distance to stars. For biographical information and a summary of Bessel's scientific work see (Fricke 2008).

Poisson was a French mathematician, a student and protégé of Laplace, an engineer and a physicist whose pendulum work, pertinent for this thesis, was a small portion of his scientific output. He made important contributions to mechanics, on which he wrote a widely used book. He also contributed to the fields of electricity, magnetism and optics, and made fundamental contributions to mathematics. In optics he was originally a defender of the particle theory of light but subsequently acknowledged the experimental verification of wave theory of light. For biographical information and a summary of Poisson's scientific work see (Costabel 2008).

There were several important experimentalists and explorers who made pendulum measurements either in a laboratory or out in the field. It would be hard to overstate the importance of the pendulum in nineteenth century physics. Pendulum measurements in the laboratory were important in preparing for fieldwork, for timing other scientific experiments, and for establishing standards of measurement, including the standard of length. Pendulums were taken around the world to make local measurements of the gravitational constant from which the exact shape of the earth was determined. The density of the earth in various locations was also determined using pendulums as part of an effort to understand the composition of the earth.

Francis Baily and Henry Kater (1777-1835) were both pivotal, British experimentalists who spent considerable time making pendulum measurements in the lab and working to design increasingly more accurate pendulums that could reasonably be used in the field. Kater was a natural philosopher who started his career in the army, where he participated in the Great Trigonometric Survey of India. Upon retiring from the army in 1814, he turned to scientific research which often consisted of the design and improvement of scientific instruments. His most important contribution was in pendulum design — this work was vitally important for accurate measurements.

Sir Edward Sabine (1788-1883) was a multifaceted Irish scientist and explorer who travelled widely and used pendulums in the field. President of the Royal Society from 1861-1871,

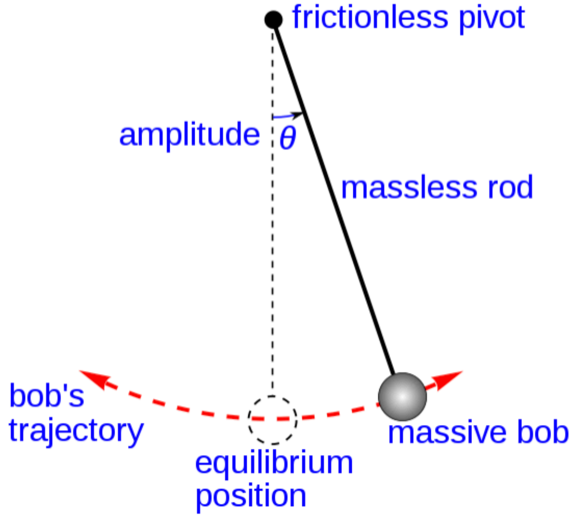

Figure 2.1: An idealized, mathematical model of the pendulum (Wikipedia contributors 2023b)

Sabine's interests ranged widely — from geophysics to ornithology — but terrestrial magnetism commanded most of his attention. He went on many explorations around the world on which pendulum measurements were made. The 1821-22 pendulum expedition around the Atlantic to determine the eccentricity of the earth earned Sabine the Copley medal. There are three islands off the coast of Greenland known as the Pendulum Islands where Sabine made pendulum measurements in 1822. The largest of these islands was named after Sabine by Carl Christian Koldewey, a German Arctic explorer who led both of the German North Polar Expeditions of the mid-nineteenth century. For further information about Sabine see (Reingold 2008).

2.2.3.2 The Idealized Pendulum and the Determination of the Period of the Pendulum

An idealized model of the pendulum, as shown in Figure 2.1, consists of a pendulum bob swinging from a pivot on a rod. The pivot is considered to be frictionless, the rod is considered to be massless, and the pendulum bob is a massive point mass. Analyzing the forces on this idealized pendulum is a standard application of basic mechanics and yields an exact solution for the period of the pendulum in the form of an infinite series,

$$T = 2\pi\sqrt{\frac{L}{g}}\left(\sum_{n=0}^{\infty}\frac{(2n)!}{2^{2n}(n!)^2}\sin^{2n}\left(\frac{\theta_0}{2}\right)\right)$$

where the first few terms of the series are

$$T = 2\pi\sqrt{\frac{L}{g}}\left(1+\frac{1}{16}\theta_0^2+\frac{11}{3072}\theta_0^4+...\right).$$

If the assumption is made that the initial displacement angle, θ, is small, then the period of the pendulum is well approximated by the formula

$$T \approx 2\pi\sqrt{\frac{L}{g}}$$

which is independent of the initial displacement angle and independent of the mass of the bob.

The simplifying assumptions built into this model, however, cause significant differences between the calculated period of a pendulum and the actual period of a pendulum as measured in a laboratory. Several sources of error result from not taking into account the real physical situation — there is friction at the pivot, the rod is not massless and its length may change with temperature or humidity, the pendulum bob is not a point mass and may not have uniform density, and most certainly will be slowed by drag in the medium through which it is swung. Further, temperature, altitude and humidity change the drag characteristics of the medium through which the bob is swung. A further source of error is the mathematical approximation made in order to make computation of the period possible. There is a loss of accuracy due to the truncation of the infinite series and that error is a function of the initial displacement angle which itself may be difficult to set repeatedly.

2.2.3.3 A Compressed History of the Pendulum to 1825

Galileo (1564-1642) is credited with the insight that pendulums can be used for timekeeping. This is despite drawings of pendulums in the work of Leonardo da Vinci (1452-1519) who appears to have not realized this application of the pendulum. In the first years of the seventeenth century Galileo found that the period of the pendulum was: approximately independent of the amplitude of the swing, independent of the mass of the bob, and proportional to the square root of the length. This allowed him to use pendulums in simple timing applications including the measurement of heart rate (Yoder 1988).

The first pendulum clock, from 1658, shown in Figure 2.2, was designed by Christiaan Huygens (1629-1695) during the mid-seventeenth century. There is some uncertainty as to whether this is actually the first pendulum clock as most sources claim or is rather a slightly later design of Huygens. For a discussion of this, see (Whitestone 2012).

In Huygens' design, the swinging of the pendulum bob was captured via a gearing mechanism which allowed for the number of swings to be counted and displayed. By 1666, Robert Hooke (1635-1703) had suggested using the pendulum to calculate longitude at sea. As we have seen, this was one of the tremendously important applications for pendulum measurements during the nineteenth century.

In 1673, Huygens answered an important theoretical question in time measurement when he found the curve a falling object must trace out if it is to move from an arbitrary initial position to a given final position in the same amount of time while under the influence of gravity alone. The solution to this tautochrone problem is the cycloid whereas, by design, a bob pendulum swings along the arc of a circle.

In order to force a pendulum bob to move along a cycloidal path, it must be guided and this creates friction — a source of error. However, the amount of error caused by the friction between the bob and a guiding surface is more than the truncation error introduced into the pendulum computations in the case where the swinging pendulum bob traces a circular arc in a vacuum. Further, the truncation error introduced by rejecting the terms containing theta from the infinite series for the pendulum period can be controlled by keeping theta, the initial pendulum displacement angle, small. The truncation error is reduced to zero as the displacement angle is made infinitesimally small and therefore it was common practice to compensate for the displacement angle by applying a reduction to infinitesimally small displacement angle correction factor to the calculation of the pendulum period.

An observed major source of error during the late seventeenth and early eighteenth century was caused by changes to the length of the pendulum rod during operation as a result of temperature fluctuations. A variety of people worked on correcting this issue in several different ways. One method was to use varnished wood for the pendulum rod to minimize changes in the rod length due to temperature. A more robust method to correct this type of error was done mechanically. In 1721 George Graham (1673-1751) used a pendulum bob filled with mercury so that as the pendulum rod expanded with temperature, the mercury also expanded raising its centre of mass towards the pivot. These two effects cancelled one another out. In 1726 John Harrison (1793-1776) invented the most widely used gridiron pendulum for temperature compensation as illustrated in Figure 2.3. The yellow bars (as shown in the figure) are made of one metal and the blue of another with

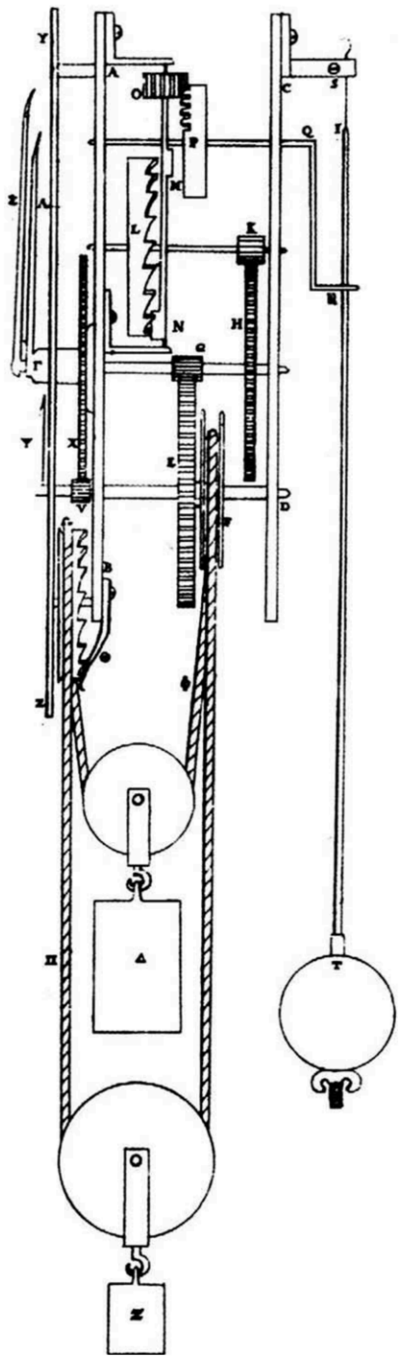

Figure 2.2: The first pendulum clock (Whitestone 2012, p.102)

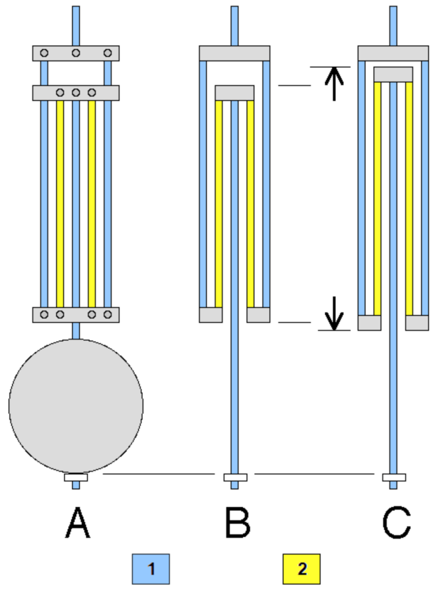

Figure 2.3: Illustration of a Pendulum with Mechanical Temperature Compensation (Wikipedia contributors 2023b)

the two metals chosen such that their thermal expansion characteristics cancel one another and the length of the pendulum rod remains constant as the temperature fluctuates. The cancellation, via this method, of error due to temperature fluctuation resulted in precision pendulum clocks whose error was in the order of a few seconds per week.

2.2.3.4 Kater and the Reversible Pendulum

In 1818, Kater designed an entirely new type of pendulum called a reversible or compound pendulum. Kater was not the first to do this. Both Gaspard de Prony (1755-1839), in 1800, and Johann von Bohnenburger (1765-1831), in 1811, proposed a reversible pendulum for the measurement of g, the gravitational constant (Candela, Martini, Krotkov & Langley 2001, p.714). However neither work was accepted or published until the late nineteenth century. The compound pendulum was a significant improvement over the simple pendulum and it is substantially different. The simple pendulum is a physical approximation to the conceptual mathematical object — an instrument consisting of a point mass suspended by a weightless, inextensible cord from a frictionless fixed support.

A compound pendulum, by contrast, is an extended solid body which vibrates about a fixed axis under the weight of the bob (see Figure 2.4 and Figure 2.5). The pendulum oscillates about the XX axis, weight W is fixed and weight A is adjustable and can be moved

Figure 2.4: Compound Pendulum
(Purdue Physics and Astronomy 2023)

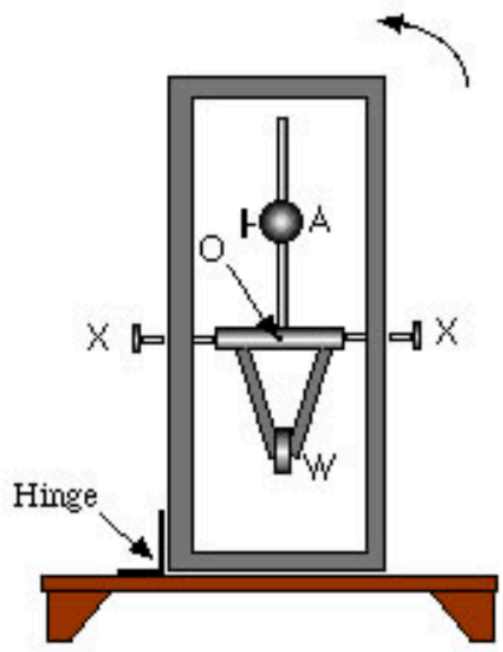

Figure 2.5: Compound Pendulum Graphic
(Purdue Physics and Astronomy 2023)

up and down the pendulum rod. This changes the period of pendulum and allows for it to be adjusted or calibrated to local conditions. Since no pendulum rods are massless, all physical pendulums are compound pendulums.

Now consider a compound pendulum where the pendulum rod is intended to have significant mass and place two knife edges at different distances and on opposite sides of the centre of gravity of the pendulum rod. This is a convertible compound pendulum. See Figure 2.6 for a photograph of a Kater compound pendulum and Figure 2.7 for an illustration of the important parts of a compound pendulum where (a) is the opposing knife edge pivots from which the pendulum is suspended, (b) is the fine adjustment weight, (c) is the course adjustment weight, (d) is the bob and (e) are the points for reading.

A convertible pendulum can be swung from either knife edge, each of which is designed to provide minimum friction at the suspension point about which the pendulum swings. If a convertible pendulum is first swung from one knife edge and then swung from the other

Figure 2.6: Kater Compound Pendulum
(Wikipedia contributors 2023b)

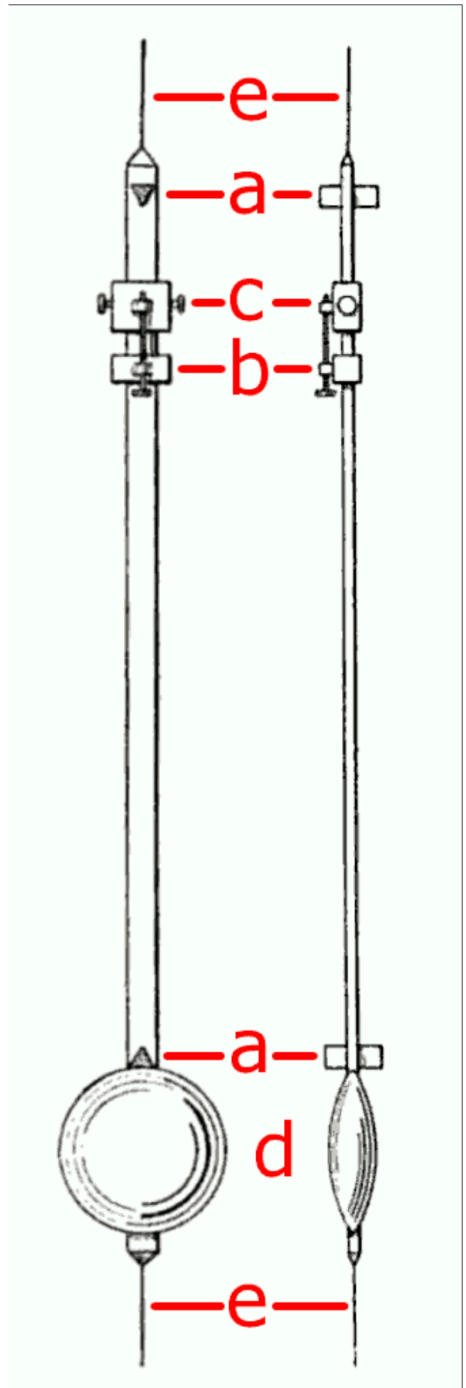

Figure 2.7: Kater Pendulum Graphic
(Wikipedia contributors 2023b)

knife edge, it is possible to adjust small weights (see Figure 2.7) to ensure the period of the pendulum is the same when swung from either knife edge. At that point, the period of the pendulum is identical to that of a simple pendulum with length equal to the distance between the two knife edges. This method does not result in a symmetric mass distribution. See (Candela *et al.* 2001) for an explanation of the physics of the Kater pendulum.

It was the Kater pendulum, designed for the absolute determination of the value of the gravitational constant at a given point, that formed the basis of English pendulum work in the early nineteenth century. Thirteen of these pendulums were constructed and these were used by Sabine between 1820 and 1825 in a variety of locations around the world. Sometimes the experimentalists used the same pendulum in multiple locations and other times identical pendulums were used in differing locations.

2.2.4 Improved Pendulum Design and Measurements by Bessel

Bessel used a simple pendulum (see Figure 2.8) in Königsberg between 1825 and 1827 to determine the gravitational constant, g, from the measured period of the pendulum. (Lenzen & Multhauf 1966, p.313) This was done by comparison to a reference clock (calibrated by astronomical observation). As seen in Figure 2.9, the pendulum being used to determine g is swung in front of a calibrated pendulum clock. The number of coincidences in position between the pendulum being used to measure g and the reference pendulum clock are counted. This is done by eye and can be observed directly or via a telescope. The telescope had the advantage that the viewer was further away from the swinging pendulums and thus would not interfere with the movement of the pendulums. The period of the detached (non-clock) pendulum can be determined very accurately by the number of coincidences with the clock pendulum over an extended period of time.

Bessel also took care to separate the clock from the pendulum so that there was no interference. He also corrected for the stiffness of the wire and the lack of rigidity of connection between the bob and the wire. Further, Bessel corrected his measurements for the buoyancy of the pendulum in the air and for the inertia of the air set in motion by the pendulum. This was done by using correction factors that were applied subsequent to the measurements.

Later, Bessel improved upon the Kater pendulum in a manner that made it unnecessary to correct for the buoyancy of the pendulum in the air and for the inertia of the air set in motion by the pendulum (Candela *et al.* 2001, p.714). Bessel observed that if the pendulum was made with a symmetric volume distribution (despite the asymmetric mass distribution) that the atmospheric effects being corrected for in the Kater pendulum cancelled out. These

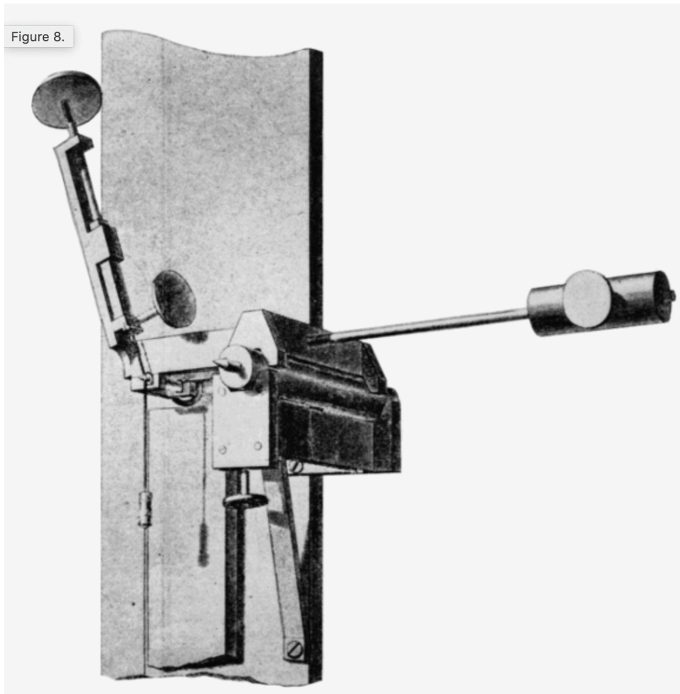

Figure 2.8: Improved simple pendulum design by Bessel (Lenzen & Multhauf 1966, p.313)

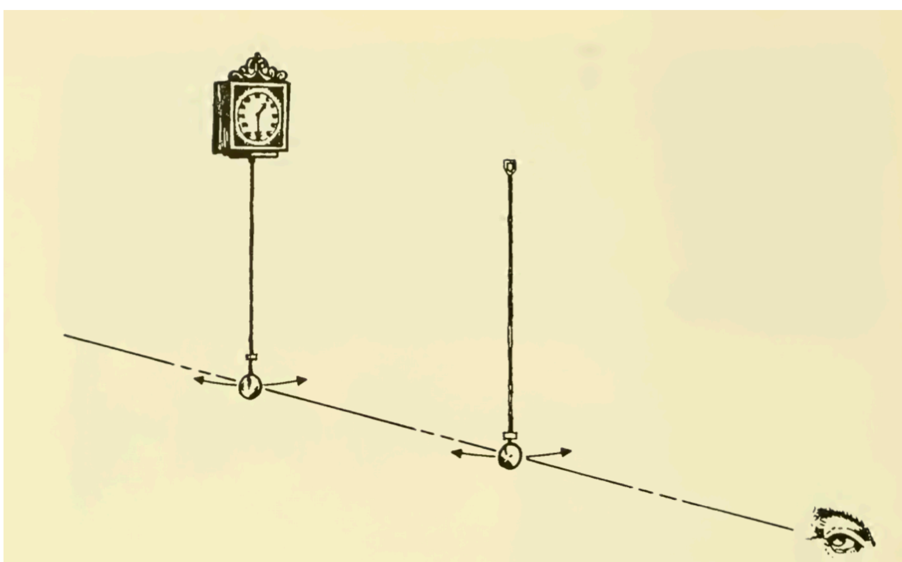

Figure 2.9: Method of coincidences (Lenzen & Multhauf 1966, p.308)

types of pendulums are sometimes called the Repsold-Bessel or Bessel-Repsold reversible pendulums as they were first manufactured by the firm by the firm A. Repsold and Sons beginning in 1861 (Candela *et al.* 2001, p.720).

2.2.4.1 Sabine's Observations and Baily's Precise Pendulum Measurements

Upon his return to England, Sabine published three papers on pendulums in the 1829 issue of *Philosophical Transactions of the Royal Society of London.* The most germane of these papers, for our purpose, is *On the Reduction to a Vacuum of the Vibrations of an Invariable Pendulum* (Sabine 1829). The other two papers are titled *Experiments to Determine the difference in the Number of Vibrations made by an Invariable Pendulum in the Royal Observatory at Greenwich, and in the Hours in London in which Captain Kater's Experiments were made* and *On the Reduction to a Vacuum of Captain Kater's Convertible Pendulum.*

Sabine's papers show that he clearly felt that the vacuum to air correction factor was not possible to establish from theory, that Bessel had published that this was a problem, and that the current experimentally determined values were deficient. Sabine found that his experimental reduction to vacuum exceeded the reduction that it was customary to compute by 3.845 vibrations per day.

Sabine discovered that Bessel had published (in January 1828) on the problem with the vacuum to air correction factors in *Astronomische Nachrichten.* Sabine said:

> "he [Bessel] has found the theory incorrect, according to which it has been customary to reduce the vibrations of a pendulum in air, to the corresponding vibrations in a vacuum: the incorrectness consisting principally, in no provision having been made in the theory, for the expenditure of the moving force, on the particles of the air set in motion by the pendulum in its vibration." (Sabine 1829, p.207)

Sabine further noted that experimental evidence existed that showed that the currently calculated correction factors were inadequate. He said:

> "It is now considered, therefore, as established by the experiments, that the true reduction to a vacuum is considerably greater than it had been customary to suppose; for the invariable pendulum, for example, nearly as 5 to 3. It was also obvious, that all pendulums whatsoever, employed in air, and designed to give results which should be independent of the variable retardation occasioned by their vibration in air, would require to have the influence of the air on their

respective vibrations, ascertained by experiment, since it is not attainable by calculation." (Sabine 1829, p. 219)

Sabine's experience led him to postulate that there may be a property of the medium through which the pendulum was swung that was currently unaccounted for and that the pendulum was the perfect experimental device to help find that. Sabine conjectured as follows:

> "May it not indicate an inherent, property in the elastic fluids, analogous to that of viscidity in liquids, of resistance to the motion of bodies passing through them, independently of their density, a property, in such case, possessed by air and hydrogen gas in very different degrees; since it would appear from the experiments, that the ratio of the resistance of hydrogen gas to that of air is more than double the ratio following from their densities. Should the existence of such a distinct property of resistance, varying in the different elastic fluids, be confirmed by experiments now in progress with other gases, an apparatus more suitable than the present to investigate the ratio in which it is possessed by them, could scarcely be devised: and the pendulum, in addition to its many important and useful purposes in general physics, may find an application for its very delicate, but, with due precaution, not more delicate than certain, determinations, in the domain of chemistry." (Sabine 1829, p. 232)

By the end of the 1820s, Baily was working on establishing the precise length of a seconds pendulum which is a pendulum with a half period of exactly one second. He was aware (through the work of Bessel) that air resistance had not been completely taken into account and so had set up a vacuum apparatus in his house to measure a variety of pendulums made of various materials both while swinging in air and in a vacuum.

By measuring the period of a pendulum in the vacuum apparatus and then measuring the same, or an identical pendulum in air, it was possible to compute a vacuum to air correction factor — that is, the factor by which the period of the pendulum changed between when it was swung in air as compared to when it was swung in a vacuum. It was not practical at that time to have vacuum apparatus in the field. This meant that the effect of the air in which the pendulum swung, further complicated by issues of varying temperature, humidity, and elevation, was a significant challenge.

The number of vibrations of two different pendulums or the same pendulum in different locations were, in the mid-1820s, considered to be strictly comparable if certain correction factors were applied. The purpose of these correction factors was to increase the accuracy of the measurements so as to permit useful comparisons between measurements taken in different locations.

Baily knew that there was error in the correction factors — the most egregious one being the reduction to a vacuum which Bessel had identified as very defective (Baily 1832, p.399). Other correction factors were somewhat lacking as well. Thomas Young (1773-1829), who in 1816 was secretary of a commission charged with determining the precise length of the seconds pendulum, showed that the correction for reduction to sea level was often too large. Sabine felt there were problems with the reduction to indefinitely small arcs which we saw earlier was used to account for non-zero and differing initial displacement angles. Sabine also pointed to error arising from the use of different agate planes on the same knife edge and to the effect of geological strata in the immediate neighbourhood of the pendulum.

Not only had vacuum to air correction factors been noted defective by Bessel and others, it was also unsatisfactory that there was no theory that could be used to compute or explain the observed pendulum period changes between air and vacuum. On May 31, 1832 Baily read a paper to the Royal Society titled *On the Correction of a Pendulum for the Reduction to a Vacuum: together with Remarks on some anomalies observed in Pendulum experiments* (Baily 1832). Baily argued for the importance of the subject by noting that many distinguished mathematicians and experimentalists were spending valuable time on this topic and that numerous scientific voyages, by several European countries, were being undertaken to various places around the world where pendulum vibrations (typically the number of vibrations in a mean solar day) were being used to determine the eccentricity of the earth by measuring the variation in the gravitational constant in different locations around the earth.

To illustrate how this was done, I summarize an example from Airy's 1826 textbook *Mathematical Tracts on Physical Astronomy, the Figure of the Earth, Precession and Nutation, and the Calculus of Variations* (Airy 1826). In this example, he explained how to compute the eccentricity of the earth using a method "which on account of its great facility is now very extensively used, ... that of observing the intensity of gravity in different latitudes, by means of the pendulum" (Airy 1826, p.120).

Airy's example was as follows. Let p, p', P be the length of the seconds pendulum at latitude l, l', and the equator respectively. Then,

$$p = P(1 + n\sin^2 l)$$

$$p' = P(1 + n\sin^2 l')$$

where $n = \dfrac{5m}{2} - e$ which allowed n to be determined when p and p' are measured and P, l and l' are known. This then enabled e, the eccentricity of the earth to be determined given that $m = \frac{1}{289}$. The value of m was determined via Clairaut's theorem which Airy explained:

> " $n + e = \dfrac{5m}{2}$: a very remarkable proposition, which may be thus stated: 'Whatever be the law of the Earth's density, if the ellipticity of the surface be added to the ratio which the excess of the polar above the equatoreal gravity bears to the equatoreal gravity, their sum will be $\frac{5m}{2}$, m being the ratio of the centrifugal force at the equator to the equatoreal gravity.' This is called Clairaut's Theorem." (Airy 1826, p.106)

This method was employed using data from Madras and Melville Island which yielded an eccentricity of the earth of $\dfrac{1}{300}$.

Pendulums were employed for other uses as well which Baily did not enumerate in his paper. For example, pendulums were used to determine the mean density of the earth. In the eighteenth century, English mathematician Benjamin Robins (1707-1751) discovered how to use a pendulum to determine muzzle velocity. This gunnery application is a specific example of the ability to use a pendulum to determine the transfer of momentum. This was used later in the nineteenth century to measure the elasticity of golf balls and the effect of spin on the distance a golf ball travels.

Baily continued in this same 1832 paper by noting that all of the correction factors in use were subject to some error. I focus here on the vacuum to air correction factor because Stokes accounted for this error in what became a very important paper. Baily noted that the measurements he made were not explainable by any known theory. He stated it thus:

> "But, to whatever cause the observed anomalies may be owing, I must confess that I have myself, during a long course of experiments on various pendulums, at different seasons of the year, and under a variety of circumstances, frequently met with discordancies that have baffled every attempt at explanation by any of the known laws applicable to the subject: and I believe that other persons also, who have had much practice in pendulum experiments, have occasionally met with anomalies for which they have been unable to account satisfactorily." (Baily 1832, p.400)

Then again a little later, Baily continued:

> "The amount of required correction, however, cannot (according to our present knowledge of the subject), be determined by calculation, but must, in every case, be ascertained by actual experiment." (Baily 1832, p.402)

Not only did Baily feel that there were anomalies which were unaccounted for but he also questioned whether there had ever been any assurance that any comparison of pendulum results were strictly valid. He said:

> "so that we are, in fact, at the present moment, totally ignorant whether the results of any two pendulums that have ever yet been constructed, are in strict and reasonable accordance with each other. And until this is practically accomplished, and can be practically repeated, I do not think that the true length of the seconds pendulum can be considered as satisfactorily determined." (Baily 1832, p.400-401)

Bessel, in 1826, showed that the usual formula for the reduction to a vacuum correction factor, based on the specific gravity of the moving pendulum bob, was very defective and did not account for the air that was set in motion by the pendulum, or for the air that adhered to the pendulum bob. This was the reason (the most direct one according to Bessel) that the pendulum must be swung in a vacuum. Bessel however did not take the direct approach he advocated; rather he swung, in air, two equal diameter spheres of different material (brass and ivory) and the same sphere in air and then in water. These experiments led Bessel to believe that the vacuum to air correction factor was perhaps double what had previously been assumed (Baily 1832, p.402).

Baily stated that the required correction factor could not be determined by calculation because of a lack of knowledge of how to do this. The alternative, then, was to measure the correction factor and so Baily proceeded to build a vacuum apparatus in his own home so he could "pursue the subject at leisure" (Baily 1832, p.403). Baily described the apparatus he used in detail. He positioned the pendulum and surrounding equipment in a specific location in his home to minimize temperature fluctuation throughout the day and throughout the year. The vacuum apparatus was mounted to minimize vibration so that external vibrations did not affect the vibrations of the pendulum. The pendulum vibrations were observed through a glass plate. From the detailed descriptions of the care taken with the experimental apparatus, the effort that Baily took to obtain accurate measurements is apparent. Indeed this is another example of the desire for precision.

As of 1826, there was a formula available to correct the number of vibrations of a pendulum used in air to what would be expected of the same pendulum swung in a vacuum. This can be considered to be a theoretical prediction of the vacuum to air correction factor though it did not come directly from theory but was rather based on empirical evidence.

To compute an air to vacuum correction factor, Baily used the formula below. He did this for many pendulums and then he took measurements of those pendulums in air and in a vacuum in order to compare the measurements to what was calculated using the formula.

I include the formula here to show the physical parameters that were considered relevant. The formula below, where N is the number of vibrations made by the pendulum in a mean solar day, computes the number of vibrations to add (as indicated by the + sign) as a result of a variety of physical parameters. S is the specific gravity of the pendulum mass and obviously varies from pendulum to pendulum. The remaining symbols represent physical constants that are pendulum independent and include: the specific gravity of air (σ), the expansion of mercury (μ), the expansion of air for a one degree increase in temperature (α), the height of the barometer (β and β'), the temperature of the mercury (τ and τ') and the temperature of the air (t and t') where the non-prime values are at standard pressure and temperature and the prime values are at the pressure and temperature at the time of the experiment.

$$+N * \frac{1}{2(\frac{S}{\sigma} - 1)} * \frac{\beta'}{\beta[1 + \mu(\tau' - t)]} * \frac{1}{1 + \alpha(t' - t)}$$

If the pendulum mass was not homogeneous then a vibrating specific gravity of the pendulum was computed which represented the net effect of the combination of all of the substances of different specific gravities that composed the pendulum bob. Baily credited Airy with that computation which also took into account how far from the axis of rotation each particular substance was. It was the calculation of the vacuum to air correction factor from the above formula that Baily found deficient.

Baily initially swung forty-one (later increased to more than eighty-four) different pendulums in air and in a near vacuum to determine a required multiplicative factor needed in each case to match the measured difference in the number of pendulum vibrations in a solar day in air and in a vacuum. This was compared with the calculated air to vacuum correction factor obtained from the above formula.

In his 1832 paper, Baily described in detail each of the pendulums selected for the experiment to strengthen the claim that almost every variety of pendulum was tested. He took a single pendulum, measured it in air, then in vacuum, twice, and then in air again in an attempt to minimize fluctuations that may occur over time.

In the end, Baily's results did not agree with those of Bessel. Even though Airy had suggested to Baily how to calculate the weight of the adhesive air, the correction factor (in effect the error in the value computed by the above formula) to the calculated value of the air to vacuum correction factor was of the order of 2 and depended on the type of pendulum with the correction factor ranging from 1.58 to 2.827. Given this range, Baily concluded that it was not possible to compare experiments using pendulums measurements unless precisely the same kind of pendulum was used.

For the pendulums used in astronomy, there were various mechanical methods to correct for vibration changes due to pressure changes, for example changing the arc of vibration, or attaching a syphon barometer to the rod of pendulum. A syphon barometer consisted of a U shaped tube that was sealed at one end and filled with mercury. At the unsealed end, changes in atmospheric pressure caused the mercury to move and form a partial vacuum at the sealed end. This changed the length of pendulum rod as the air pressure changed. These methods of mechanically compensating for error were insufficient, a fact noted by both Bessel and Poisson.

In supplementary experiments appended to Baily's paper, Baily noted that Airy took a lively interest in precise pendulum measurements and that he made useful and motivating suggestions and recommendations. Airy said:

> "It appears that the phenomena, to which you allude, may generally be explained by supposing a quantity of air, depending on the figure of the body, to adhere to it whilst it is moving, and to add to its inertia without altering its gravitation." (Baily 1832, p.440)

Baily did a series of experiments to try to find the quantity of air adhering to, or being dragged by the pendulum bob of various shapes including a sphere, cylinder and disc. The essential conclusion was that air was adhering to the pendulum in a non-uniform way and that this was affecting the period of the pendulum in a way that was not understood or predicted by theory. Baily said:

> "It appears then that all these results accord with the theory that a quantity of air adheres to every pendulum when in motion: and, by thus forming a portion of the moving body, diminishes its specific gravity; or, rather adds to its inertia. This adhesive air is confined almost wholly to the two opposite portions of the pendulum, which lie in the line of its motion; (similar to what takes place with a body moving through still water), and very little of it adheres to, or is dragged by, the sides of the pendulum. The shaping of air will consequently partake, in some measure, of the form of the pendulum; subject probably to some slight modifications, with the nature of which, however, we are at present unacquainted. The quantity of air dragged by a pendulum seems to depend on

> the extent and form of surface opposed to its action, and is not affected by the density of the body." (Baily 1832, p.456)

Baily foremost credited Bessel for this observation, which was, in 1832, not calculable from theory. Interestingly Baily also cited the work of Pierre-Louis-George du Buat (1734-1809) (du Buat 1786) from nearly 50 years earlier and pointed out that the effect of the medium on the motion of the pendulum was already known at that point and had, despite initial interest, been largely forgotten. Baily said:

> "But, is it not a remarkable circumstance in the history of this subject, that these important and apparently conclusive experiments of M. Du Buat, [...] is it not singular that such experiments should have been so soon and so completely lost sight of, and forgotten, that not one of the many distinguished individuals actually engaged in those pursuits, and in the investigation of this subject, should have had the least idea or remembrance of the additional correction for the reduction to a vacuum so clearly pointed out by M. Du Buat: and that until the re-discovery of this principle by M. Bessel, as detailed in his valuable paper on the pendulum, no one should have thought of verifying the suspicion of Newton that such an effect was probable." (Baily 1832, p.460)

The Baily paper ended with many pages of tables of experimentally determined correction factors for a large number of different pendulums.

Thus, by 1832, there was a large volume of very careful measurements of pendulum motion available and there was recognition that there was no theory available to generate the correction factors. All theory prior to 1848 was simply unable to explain the experimental results. We see next how these precise pendulum measurements lead to new physical theory in the hands of Stokes.

2.3 Stokes and Pendulum Theory

It is with a lack of theory capable of predicting pendulum correction factors but with a large number of experimental results that Stokes turned his attention to the problem. At some point prior to December of 1850, Stokes met with Sabine, now Col. Sabine at William Hallowes Miller's house and pendulum experiments were discussed. Stokes, in a letter of December 6th, 1850, recalls the above meeting with Sabine (Stokes 1907a, p.253), which indicates that he had read Sabine's paper *On the Reduction to a Vacuum of the Vibrations of an Invariable Pendulum* published in 1829.

Stokes also reported in the 1850 letter to Sabine that he had not seen further notice about Sabine's observations that the change in the period of the pendulum in differing media (in this case, air, a near vacuum and hydrogen) was not caused solely by variation in density. Stokes stated:

> "the experiments showed that the retardation could not at all be inferred from the density, in passing from one elastic fluid to another." (Stokes 1907a, p.253)

Thus, during the 1840s Stokes read Sabine's paper of 1829 and he likely read all three of Sabine's papers since they were in the same issue of the same journal. He discussed the situation in person with Sabine and he noted that there had not been any follow up publication on finding a solution to the vacuum to air correction factor determination.

Stokes continued in the 1850 letter to tell Sabine that he had effectively solved the problem and that he had read a paper at the last meeting of the Cambridge Philosophical Society which he hoped to shortly send to him. That paper, which we will look at in detail shortly, contained, as Stokes explained:

> "the calculation of the resistance to a pendulum in the two cases of a sphere and of a long cylindrical rod, when the *internal friction*, as it may be called, of the fluid is taken into account. The agreement of theory with Baily's experiments is very striking." (Stokes 1907a, p.253)

In a second letter of January 10, 1851 to Sabine, Stokes thanked Sabine for a reply to the 1850 letter. From the second letter, it is apparent that Sabine's reply must have indicated to Stokes that additional experiments had been performed on the action of different elastic fluids on the vibration of the pendulum. Stokes told Sabine that he was already convinced that the pendulum motion was affected by the media in a manner that was independent of the media's density. He based that on the two hydrogen experiments. Further, Stokes told Sabine that his new theory produced numbers that agreed well with Baily's experiments even though Baily's numbers did not agree with the results generated by the common theory of fluid motion. Stokes said:

> "though Baily's results are at variance with the common theory of fluid motion, in which the pressure is supposed equal in all directions in a fluid, or, which comes to the same, in which the fluid is supposed to be perfectly smooth, they agree beautifully with the formulae to which I have been led by employing a theory in which what may be called the internal friction of the fluid is taken into account." (Stokes 1907a, p.254)

> taken into account. In this theory it is supposed that a continuous sliding motion of the fluid calls into play a tangential pressure proportional to the rate of sliding. Thus, imagine a fluid to flow in horizontal layers, the velocity increasing uniformly from the ground upwards, so that the layers of fluid which are at one time arranged like ═ after a certain time come to be arranged like ⫽; then I suppose that, if you draw any imaginary horizontal plane through the fluid, the motions of fluid above and below the plane act tangentially on one another thus ⇄. The tangential pressure for a sliding unity, referred to a unit of surface, and divided by the density, is a constant depending upon the nature of the fluid, which I propose to call the index of friction of the fluid.

Figure 2.10: Stokes to Sabine on the conception of the index of friction (Stokes 1907a, p.254)

Figure 2.10 is an image of a portion of Stokes' 1851 letter to Sabine. The text in this image, along with the accompanying drawings, makes it clear how Stokes was physically thinking about the problem. Stokes was thinking of the medium surrounding the pendulum bob as consisting of layers which moved tangentially to one another as the pendulum bob moved. Stokes said that the layers of fluid through which the bob moved changed from stack of layers one on top of the other to a stack of layers where the layers closest to the pendulum had moved more than those further away as the pendulum swung. And that that movement of one layer upon its neighbours was a source of friction dependent on the "nature" of the fluid, not the density of the fluid.

We are dealing here with two rather large breakthroughs. The first, as this letter makes apparent, was the physical realization that there exists in fluid motion a previously unknown phenomenon — that which results in the boundary layer — which Stokes called the index of friction of the fluid. The second breakthrough (to be discussed in detail in the next chapter and not evident from this brief text), was the ability to use asymptotic approximations to divergent series in order to obtain numbers from this new theory.

Stokes indicated at the end of this letter that he would like to be able to make additional experimental measurements to determine the index of friction for different gases as well as make additional measurements on pendulum vibrations in air in order to test his new theory. I emphasize that last point — Stokes sought to verify new theoretical results by obtaining additional experimental measurements.

Stokes wrote a third letter to Sabine about pendulum accuracy in 1863. In this letter, Stokes reflected and summarized what had passed. He started by expressing appreciation for the referral to Bessel's memoir which he had obtained and for the two letters from Sabine. Stokes then summarized the methods for eliminating the effect of the air on the pendulum. These he referred to methods 1, 2, 3a and 3b.

Method 1 was the direct method of swinging the pendulum in a vacuum. This eliminated the effect of air or any other medium on the time of vibration or on the arc of vibration. This, said Stokes, was the most exact method. Clearly this is not always practically possible and, in particular, not practical for field work.

Method 2 was to swing the pendulum in air and then in vacuum, under controlled circumstances, and use the thus obtained correction factor in subsequent uses of the pendulum in air. This, said Stokes, was extremely convenient and any issues with this method resulting from temperature fluctuations were likely to be small.

Method 3 was the method of Bessel which employed a symmetric convertible pendulum. A problem with this method is that a pendulum that is convertible in a vacuum is not convertible in air. There were two ways to approach this problem: in method 3a, an adjustable weight was used but that destroyed the invariability of the pendulum and had to be done at each location. Stokes rejected this method. Method 3b) consisted of measuring the difference in vibration between the two knife edges and using that to compute (as Stokes showed how to do) the time of vibration as though the pendulum were invariable.

Stokes' interest in pendulums and pendulum experiments continued throughout his life, with a particularly large number of letters between Stokes, Sabine, James Thomas Walker (1826-1896) (referred to as Colonel Walker and appointed Surveyor General of India in 1878), and Oliver Heaviside (1850-1925) in 1872 and 1873. Stokes was recognized by that time as the British authority on the mathematical modelling of pendulums as evidenced from comments in a variety of places and from the correspondence. For example, regarding the Great Trigonometric Survey of India discussed earlier, Larmor, the editor of Stokes' correspondence, wrote a summary of the letters on the topic of the Indian survey, pendulums, and gravity measurement. That summary makes clear that Stokes' work on the pendulum and his discovery of internal friction mid-century established him as the leading British expert on geodetic matters. Larmor said:

> “The early preoccupation of Sir George Stokes with the reduction of the existing classical pendulum observations, in connexion with his great memoir on the resistance offered by viscosity of the air, and the illumination which his memoirs on Clairaut’s Theorem and the Figure of the Earth threw on the connexion between the form of the sea level and the distribution of gravity, naturally constituted him throughout his life the first British authority on the principles of all geodetic operations. The pendulum observations of the Great Indian Trigonometrical Survey, the results of which have taken so prominent a position in the Science of Geodesy, thus occupied a large share of his attention, both officially at the Royal Society and in the way of private discussion with the directors of the operations. A large correspondence exists, first with General Sabine, and later with General J. T. Walker, Col. Herschel, Capt. Heaviside, and other officers of the Indian Survey.” (Stokes 1907a, p.271)

These letters contain brief mentions of Stokes’ method of divergent series with the computational results succinctly summarized. For example, in a July 25, 1872 letter to Walker, Stokes said:

> “Many years ago I investigated the problem of the resistance of a fluid to a pendulum, taking into account the internal friction of the fluid itself. A very tough problem it was, but I succeeded in obtaining the solution in the case of a sphere, and in that of a cylindrical rod of which each element was treated as an element of an infinite cylinder vibrating with the same linear velocity and without change of direction of the axis. I will send you by book post a copy of my paper if you have not got it, but I think you have.” (Stokes 1907a, p.275-276)

I will revisit this below when I look at the impact of Stokes’ mathematics on asymptotic expansion of divergent series following the publication of his 1850 paper.

2.3.1 Stokes’ 1848 Paper on Air Resistance and Pendulums

In 1847, Stokes reported his new results on pendulum vibrations at a meeting of the British Association at Oxford. These results were published in a short two page note, titled *On the Resistance of the Air to Pendulums* (Stokes 1848), in the 1848 volume of the reports. In this note Stokes stated that he had obtained theoretical results for computing the vibrations of spherical pendulum bobs and cylindrical pendulum bobs.

Stokes used the common theory of hydrodynamics as previously obtained via different methods by Claude-Louis Navier (1785-1836), Poisson, and Adhémar Jean Claude Barré de

Saint-Venant (1797-1886). For a history of hydrodynamics during this time see *Worlds of Flow: a History of Hydrodynamics from the Bernoullis to Prandtl* (Darrigol 2005). In this book, Darrigol emphasized the importance of hydrodynamics in the physical sciences and chose as his opening sentence an 1857 quote from Thomson to Stokes in which Thomson said:

> "Now I think hydrodynamics is to be the root of all physical science, and is at present second to none in the beauty of mathematics." (Darrigol 2005, p.v)

Darrigol enumerated the many worlds of flow to which hydrodynamics was relevant. These included flow in rivers and canals, blood and sap flow and, important here, the damping of the seconds pendulum in air. Hydrodynamics was also important to the British theory of the ether as a perfect liquid.

Further Darrigol identified a divide between theory (called hydrodynamics) and practice (called hydraulics) with the difficulty that hydrodynamical theory often predicted phenomena that were in opposition what was actually observed or done in practice by the men of hydraulics. Darrigold said:

> "Whereas hydrodynamicists applied advanced mathematics to flows rarely enountered by engineers, hydraulicians used simple empirical or semi-empirical formulas that defied deeper theory." (Darrigol 2005, p.vi)

In the long and complex history of hydrodynamics in the nineteenth century are parallels to what I discuss in this thesis. There are theoreticians and there are practitioners. The theory evolves over time. The empirical results accumulate and intuition is developed that allows for corrections to calculations or for designs to be improved. The measurements inform the theory and the theory informs the design of the measuring apparatus.

The computations Stokes made for spherical and long cylindrical rod pendulum bobs were simple. For a spherical bob, the mass must be increased by half, and for a cylindrical bob the mass must be increased by the whole of the mass displaced by the bob. This additional mass increased the inertia of the pendulum but not its weight and in these two cases measurements agreed with the results of du Buat for spheres oscillating in air and with the results of Baily for cylinders of one and half inches in diameter.

However, with smaller spheres or thinner rods, the computations from hydrodynamics, which Stokes published in the eighth volume of the *Cambridge Philosophical Transactions*, no longer agreed with experiment. As we have seen, the experimental work of Bessel and Baily made this evident.

The case of the sphere was a straightforward calculation from the refined hydrodynamical theory Stokes developed but, in the case of the cylinder, it was difficult to compute numerical results from the theory. Stokes said that the theory, in the case of the cylindrical bob, resulted in a convergent ascending series (meaning in increasing positive powers of the variable) involving the derivative of the gamma function. As noted, the Stirling series, an asymptotic approximation to the gamma function, was stated by de Moivre well before this. It is possible that seeing the derivative of the gamma function caused Stokes to consider trying to find an analogous approximation method.

In the case of the cylinder, Stokes then applied his new mathematical tool which consisted of converting the convergent series to a descending divergent series, from which he used the first few terms as an approximation. Stokes used the contemporaneous term "descending series" to refer to series where the power of the variable increases in the denominator as the terms progress. This simplified, in fact made possible, the numerical calculations. Stokes said:

> "the author has also obtained a descending series, which is much more convenient for numerical calculation when the diameter of the cylinder is large." (Stokes 1848, p.7)

Stokes' 1848 note concluded with a summary of the important formulae he had obtained but did not indicate of how the formulae were obtained. He also provided three numerical calculations for three different sized cylinders. These calculations were compared against previously obtained standard results as well as with the experimental results of Baily. Stokes' new theory agreed much better with the experimental results of Baily than the previously obtained theoretical predictions which did not account for internal friction did. Here for the first time, as far as I know, Stokes is announcing his use of a divergent series for computation with respect to pendulums.

The most important of Stokes' papers on pendulums was read on December 9, 1850, about two years after the British Association report, and was then published in volume nine of the Transactions of the Cambridge Philosophical Society in 1856. During this time, Stokes also sent his paper, titled *On the Effect of the Internal Friction of Fluids on the Motion of Pendulums* (Stokes 1907a), via post to interested parties. This is a very long paper, running to 99 pages, focussed on the "reduction to a vacuum" correction factor. The introduction to this paper provides us with an example of the importance attached to precision by both Stokes and his readers. Stokes used the words "modern exactness".

He stated that pendulums were important for deducing results about physical phenomena and thus there had been a lot of time and energy spent on making accurate pendulum measurements. Stokes said:

> "The great importance the results obtained by means of the pendulum has induced philosophers to devote so much attention to the subject, and to perform the experiments with such a scrupulous regard to accuracy in every particular, that pendulum observations may be justly ranked among those most distinguished by modern exactness." (Stokes 1856a, p.8)

What was meant here by "modern exactness"? One meaning is that a measurement of an actual pendulum in a particular situation can be corrected for comparison with what a simple pendulum performing indefinitely small oscillations in a vacuum would do. Perhaps it also meant that Stokes' theory yielded results that agreed with experimental measurements, mostly as performed by Baily.

In the first few pages of the 1856 pendulum paper, Stokes outlined the history of computation of the pendulum correction factor for reduction to a vacuum. First, he pointed to the important 1828 paper of Bessel in which Bessel pointed out the theoretical considerations of the necessity of taking account of the inertia of the air as well as of its buoyancy. Bessel did not make a numerical calculation of the effect of inertia. He did however conclude that the effect of buoyancy and inertia were the only effects of a fluid of low density on the pendulum movement.

The Commissioners for the Discovery of the Longitude at Sea, or more popularly the Board of Longitude, was a British government body formed in 1714 to administer a scheme of prizes intended to encourage innovators to solve the problem of finding longitude at sea. Bessel's statements about the necessity of theory to account for the effect of both inertia and buoyancy of the air on pendulum motion spurred the Board of Longitude to build a large vacuum apparatus which Sabine used to measure the effect of air on a pendulum. This was shortly before the demise of the Board of Longitude in 1828 when it was replaced by a Resident Committee for Scientific Advice for the Admiralty consisting of three scientific advisors: Thomas Young, Michael Faraday and Edward Sabine.

These were the results, referred to in Stokes' 1850 letter to Sabine, that were read before the Royal Society in March of 1829 and published in the Philosophical Transactions of the Royal Society of London of the same year. As I discussed above, Sabine's pendulum experiments in air, in a near vacuum and, importantly, in hydrogen gas, resulted in the discovery that, at the same density, air and hydrogen affected the motion of the pendulum differently.

Then Stokes mentioned the valuable pendulum work of Baily saying:

> "Our knowledge on the subject of the effect of air on the time of vibration of pendulums has received a most valuable addition from the labours of the late Mr. Baily, who erected a vacuum apparatus at this own house, with which he performed many hundreds of careful experiments on a great variety of pendulums." (Stokes 1856a, p.9)

The results of Baily were read to the Royal Society in May of 1832. Baily used the letter n to indicate the factor by which the correction for buoyancy must be multiplied to account for the whole effect of air as measured by his experiments. These agreed with similar computations by Bessel. However, and critically, at the end of Baily's paper, in the section on additional experiments, were results with smaller cylindrical rods that showed that n increased as the diameter of the rod decreased as though "according to an unknown law" (Stokes 1856a, p.10).

That was the experimental side of the progress. Stokes mentioned du Buat's work of 50 years earlier which he felt had been completely overlooked and forgotten. There was, however, mathematical progress during the 1830s — a paper of Poisson in 1831, a paper of Challis in 1833, a paper by Green in 1833, and a paper by Plana in 1835. Then Stokes pointed to a paper of his own read in May of 1843 which handled the case of the pendulum confined in a sphere (Stokes 1856a, p.11).

Then came the mathematical idea which Stokes built upon to solve the problem. Stokes learned from Thomson, likely about 1845, of a method that was used to solve problems in electricity called the method of images and realized that he could adapt that method to the current problem in pendulum computations. The method of images is a mathematical tool for solving differential equations, in which the domain of the sought function is extended by the addition of its mirror image with respect to a symmetry hyperplane. As a result, certain boundary conditions are satisfied automatically by the presence of the mirror image which greatly facilitates the solution of the original problem. Stokes said:

> "A few years ago Professor Thomson communicated to me a very beautiful and powerful method which he had applied to the theory of electricity, which depended on the consideration of what he called *electrical images*. The same method, I found, applied, with a certain modification, to some interesting problems related to ball pendulums. It enabled me to calculate the resistance to a sphere oscillating in the presence of a fixed sphere, or within a spherical envelope." (Stokes 1856a, p.11)

Stokes stated what he was aware of when he started his investigations on the pendulum correction factors and I have summarized those above. He said:

> "The preceding [as I have summarized above] are all the investigations that have fallen under my notice, of which the object was to calculate from hydrodynamics the resistance to a body of given form oscillating as a pendulum. They all proceed on the ordinary equations of the motion of fluids. They all fail to account for one leading feature of the experimental results, namely, the increase of the factor n with a decrease in the dimensions of the body. They recognize no distinction between the action of different fluids, except what arises from their difference of density." (Stokes 1856a, p.12)

Stokes noted that none of previous theories accounted for what is now termed viscosity and it was evident from the experiments of several people that theory and experiment were not in agreement.

Further Stokes mentioned that he had been in conversation with Dr. Robinson (most likely this was his father-in-law who was the astronomer at Armagh Observatory in Ireland) about 7 or 8 years prior (that would be about 1843) and was made aware then of the unpublished results of Sir James South (1785-1867). South was a British astronomer who was joint founder of the Astronomical Society of London. South performed experiments where he attached a piece of gold leaf to the bottom of the pendulum ball so that the gold leaf was perpendicular to the surface of pendulum and perpendicular to the ground when the pendulum was at rest. He then observed the motion of the gold leaf as the pendulum swung and found that the gold leaf stayed perpendicular to the ground until the pendulum had moved a considerable distance. Stokes described South's observations thus:

> "Sir James South found that the gold leaf retained its perpendicular position just as if the pendulum had been as rest; and it was not till the gold leaf carried by the pendulum had been removed to some distance from the surface, that it began to lag behind. This experiment shews clearly the existence of a tangential action between the pendulum and the air, and between one layer of air and another." (Stokes 1856a, p.12)

This reminded Stokes of an experiment of Charles-Augustin de Coulomb (1736-1806) where a similar property had been observed. Coulomb had measured the decrement of the arc of oscillation of a disc spinning in its plane in water under the torsion of a wire and had published the results, in 1799, in an article titled *Expériences destinées à déterminer la* *cohérence des fluides et les lois de leur résistance dans les mouvemens très-lents* (Coulomb 1884, p.333).

Further, in the neglected work of du Buat, a property called the viscosity of the fluid was used to explain slight increased times of vibration in his experiments of oscillations of spheres in water. Having acquainted himself with the variety of experimental and theoretical evidence available and with new theory in hand, Stokes was able to compute vacuum to air correction factors that agreed with the experimental results of Baily.

He first tried to compute the vacuum to air correction factor for the cylindrical pendulum bob. Stokes explained:

> "I first tried a long cylinder, because the solutions of the problem appeared likely to be simpler than the case of a sphere. But after having proceeded a good way towards the result, I was stopped by a difficulty relating to the determination of the arbitrary constants, which appeared as the coefficients of certain infinite series by which the integral of a certain differential equation was expressed." (Stokes 1856a, p.12)

Stokes then put on hold the computations for the cylinder and then tried to compute the vacuum to air correction factor for a spherical pendulum bob. He said:

> "Having failed in the case of the cylinder I tried a sphere, and presently found that the corresponding differential equation admitted of integration in finite terms — the result, I found agreed very well with Baily's experiments when the numerical value of a certain constant was properly assumed." (Stokes 1856a, p.13)

At this point, Stokes had a refined hydrodynamical theory that took into account viscosity and that theory agreed with experimental evidence in the spherical pendulum case. No new mathematical techniques had been required for the computations thus far.

Stokes then temporarily put aside the pendulum computations and attempted to compute zeros of the Airy integral. The theory for this was well-known — the difficulty lay entirely in being able to obtain numbers from the theory. After he devised a new mathematical technique that he used to make the Airy integral computations, Stokes returned to the pendulum computations in the case of the cylindrical bob, and was able to use his new technique to obtain results. Stokes said:

> "I found the method which I had employed in the case of this integral [the Airy integral] would apply to the problem of the resistance to a cylinder and it

enabled me to get over the difficulty with which I had before beeen [sic] baffled. I immediately completed the numerical calculation so far as was requisite to compare the formulae with Baily's experiment on cylindrical rods, and found a remarkably close agreement between theory and observation. These results were mentioned at the meeting of the British Association at Swansea in 1848, and are briefly described in the volume of reports for that year." (Stokes 1856a, p.13)

On the Effect of the Internal Friction of Fluids on the Motion of Pendulums was devoted to the finding of the vacuum to air correction factor in two cases: a sphere and a long cylinder. The results were compared to the experiments of Baily and others. The effect of the fluid on the time of the vibration as well as on the arc of vibration were considered and a single number was produced by the theory to account for the effect of the medium on the motion of the pendulum. Stokes proposed that this effect be called the *index of friction*. This was later recognized as an epiphenomenon of viscosity.

Stokes further verified his pendulum results by using an index of friction for water taken from the results of Coulomb's experiment on a spinning disk in water. This value for the index of friction of water was not computed using a pendulum. By using Coulomb's value for the index of friction to compute the pendulum vibration period of a pendulum swinging in water, Stokes found his theory agreed with Bessel's experiments.

In addition to the ability to compute vacuum to air correction factors for pendulums, Stokes' discovery of the index of friction provided an explanation for the formation of clouds. In simplified terms, the explanation was as follows:

1. A sphere (water droplet) travelling uniformly in a fluid was considered as a limiting case of a ball pendulum as the length of the wire became arbitrarily large.

2. Stokes' theory showed that the resistance due to internal friction of a sphere moving through a fluid was proportional to the radius of the sphere rather than the surface area of the sphere.

3. The index of friction for air was known from pendulum experiments.

4. The terminal velocity of the water droplets was calculated using the index of friction for air (the other sources of friction, proportional to the square of the velocity, were much less significant and were ignored) and was so small that the suspension of water droplets to form clouds was explained.

This was, perhaps, another verification of the new theory of the index of friction.

Following the introduction to the paper (approximately 14 pages), the remaining content was divided into two parts and provided in-depth analysis first of the theory and second a comparison of the theory with experiment. Part one was the analytical investigation and it was divided into five sections:

1. an analysis of the physics,
2. the results from theory for the sphere in unlimited media or oscillating within a medium enclosed by a sphere,
3. the results for the unlimited cylinder in an unlimited medium,
4. justification and discussion of the calculations in sections 2 and 3,
5. an application to oscillatory waves.

Part two of the paper compared the theory of part one with experimental evidence and consisted of two sections. Section one discussed the experiments of Baily, Bessel, Coulomb and du Buat. Here Stokes called the work of du Buat excellent and refers to du Buat's *Principes d'Hydraulique* (du Buat 1786) in the second edition published in 1786. Section two consisted of suggestions for future experiments.

The validity of the method Stokes used is discussed further in the next chapter. At the close of this chapter, I emphasize how important this major paper of Stokes was — it elucidated a new physical phenomenon and the calculations were done with the use of a new numerical method.

As we shall see in the next chapter, when we see precisely how Stokes used divergent series to find the zeros of the Airy integral, Stokes had a method, using divergent series, to now obtain numbers from theory that were not previously practically computable which he could then compare to something that was measurable in a laboratory.

Chapter 3

Stokes and the Use of Divergent Series

In the previous two chapters I showed that it was a controversial, non-standard practice to use divergent series in the late 1840s. I have also detailed why and when Stokes was using divergent series and the milieu in which this occurred.

In this chapter I show how Stokes used divergent series for asymptotic approximations and with what justification. I include a detailed discussion of Stokes' paper that outlines the mathematical method he used in making calculations from theory both in optics and for pendulums. As we shall see, Stokes, in the paper where he used divergent series so effectively for computation, provided no discussion about whether or not the results were on firm mathematical footing given that he used a divergent series to produce numerical values. Stokes did, however, in the same time frame, publish on convergence and I look to this work to understand Stokes' views of convergence.

Hardy, in *Divergent Series* (Hardy 1949), referred to Stokes' paper on convergence, *On the Critical Values of Sums of Periodic Series* (Stokes 1849, p.533), as famous because, Hardy claimed, it was the first time the important concept of uniform convergence appeared in print. In this paper Stokes carefully discussed convergence with a particular emphasis on periodic series — series which consisted of sums of sine and cosine functions.

On the Critical Values of Sums of Periodic Series appeared in the same volume (Volume 8) of the *Transactions of the Cambridge Philosophical Society* as did De Morgan's 1844 paper on divergent series. Volume 8 of the *Transactions* was published in 1849 though the papers themselves were read earlier and were also on occasion, mailed to interested parties before publication. Hardy referred to Volume 8 of the Transactions in the following manner:

> "There is one volume of the *Transactions of the Cambridge Philosophical Society* (vol.8, published in 1849 and covering the period 1844-9) which contains a very singular mixture of analytical papers and gives a particularly good picture of the British analysis of the time. It contains Stokes' famous paper 'On the critical values of the sums of periodic series', in which uniform convergence appears first in print; papers by S. Earnshaw and J.R. Young which are little more than nonsense; and a long and interesting paper by de Morgan on divergent series, a remarkable mixture of acuteness and confusion." (Hardy 1949, p.18)

The method that Stokes used to turn convergent definite integrals that did not admit integration in finite terms into divergent asymptotic expansions was explained in his paper *On the Numerical Calculation of a Class of Definite Integrals and Infinite Series* (Stokes 1856b) read on March 11, 1850. My analysis and discussion of that paper follows the discussion of convergence paper below.

3.1 Stokes' 1847 Paper on Sums of Periodic Series

At the outset of *On the Critical Values of Sums of Periodic Series*, Stokes noted that there were various series by which an arbitrary function can be expressed between certain limits of the function argument. For example, the independent variable may vary between 0 and a for a given series representation. Of particular interest, in this paper, were the series that proceed according to sums of sines of multiples of $\frac{\pi x}{a}$ and those which proceed as sums of cosines of the same angles. These are effectively a particular kind of Fourier series.

The main points of interest of this paper, which was read December 6, 1847, are that:

1. It clearly indicates that Stokes was very carefully considering convergence during this time period.

2. Stokes made clear statements about what he meant by convergent and divergent, which I will quote below.

3. It is the first paper in which the concept of uniform convergence appears in print.

At the outset of the paper, Stokes stated that there were many problems in heat, electricity, and fluid motion which were solved by developing a function in a series or as an integral of functions of known form. According to Stokes, Fourier, in the 1822 book *Théorie analytique de la chaleur* (Fourier 2009), was the first to do so systematically.

The main object of Stokes' paper was to investigate how to properly expand an arbitrary function in a Fourier series when the value of the function at the left and right hand endpoints, which determine the form of the expansion, were violated. Stokes concluded that using series was frequently advantageous in these cases.

In the first section of the paper, Stokes proved the possibility of an expansion of an arbitrary function in a series of sine functions. After noting that there are essentially two methods to do this expansion, Stokes decided to follow a method employed by Poisson.

Poisson's method consisted in considering the series as the limit of another series formed from it by multiplying its terms by the ascending powers of a quantity a little less than one. Note here the similarity to what, as discussed earlier, De Morgan claimed to be the principle of Poisson.

Stokes' reason for this was twofold:

1. This was how series present themselves in physical problems.
2. It was the method Stokes first used when he began the investigations which led to this paper.

That periodic series appeared directly from physical problems meant that continuity and convergence properties could be tacitly assumed. This indeed may be a reason why Stokes did not find it necessary to carefully consider convergence when using his asymptotic expansions.

According to Stokes, the method of Poisson:

> "... has this in its favour, that it is thus that the series present themselves in physical problems. [It] is the method which I have followed, as being that which I employed when I first began the following investigations, and accordingly that which best harmonizes with the rest of the paper." (Stokes 1849, p.532)

Stokes' reason for providing a proof in this paper of the well-known theorem that a function can be expanded in a series of sine functions was that some mathematicians had doubts about the ability to do this and that Poisson provided insufficient detail on certain points.

Next, Stokes showed how to find the existence and nature of the discontinuities of a function and the derivatives of a function using the series from which the function was developed. We saw earlier that this was something that De Morgan claimed, in 1844, was not possible.

In general it is not possible to find the derivative of a function expressed as a series by differentiating under the summation sign as Stokes knew. He showed though how it was done when it was possible. Stokes stated that it was not necessary for the expanded function to be finite; it was sufficient for the integral of the function to be finite.

According to Stokes, some physical problems are solved by methods that results in a definite integral as the solution. Even though that may be the general method, it is possible, by using a different method, to obtain results as an infinite series. Stokes employed the second method because numerical approximations using infinite series can be obtained by summing the first n terms whereas the approximation of an definite integral directly, by using a trapezoidal approximation for example, can be much more laborious. Stokes was clearly thinking about computational efficiency.

When using a partial sum of an infinite series to approximate a solution, the convergence of the series is an essential consideration. Stokes defined divergent and convergent in the 1847 paper in the following way:

> "The terms *convergent* and *divergent*, as applied to infinite series, will be used in this paper in their usual sense; that is to say, a series will be considered convergent when the sum to n terms approaches a finite and unique limit as n increases beyond all limit, and divergent in the contrary case." (Stokes 1849, p.535)

This reads like the Cauchy definition which, when stated with symbols, is that $\sum_{i=0}^{\infty} a_i$ is convergent if and only if for every $\epsilon > 0$ there is a natural number N such that $|a_{n+1} + a_{n+2} + \cdots + a_{n+p}| < \epsilon$ holds for all $n > N$ and all $p \geq 1$.

This was not the only place that Stokes used a Cauchy definition. For example, as historian Jacqueline Stedall (Stedall 2008, p.527) pointed out, Stokes adopted the Bolzano-Cauchy definition of continuity which states that $f(x)$ is called continuous when, for all values of x, the difference between $f(x)$ and $f(x \pm h)$ can be made smaller than any assignable quantity by sufficiently diminishing h.

The Stokes definition of continuity is relevant because it is during this time that Stokes realized, and published, in this paper, that for an infinite sum of continuous functions to be continuous, the convergence must be of a particular kind. Stokes labelled this type of convergence *not infinitely slow.*

This understanding arose independently, also in 1847, in the work of Phillip Ludwig von Seidel (1821-1896). Seidel called the necessary convergence *not arbitrarily slow.* The work of Seidel and Stokes foreshadowed the work of Karl Theodor Wilhelm Weierstrass (1815-1897) on uniform convergence that appeared a few years later (Stedall 2008, p.527).

The development of the concept of uniform convergence has a complicated history in the mid-nineteenth century. See for example *The Foundation of Analysis in the* 19^{th} *Century* by Jesper Lützen (Jahnke 2003, p.155) or *Geschichte der gleichmäßigen Konvergenz: Ursprünge und Entwicklungen des Begriffs in der Analysis des 19. Jahrhunderts* by Klaus Viertel. (Viertel 2014)

According to Stokes, oscillating series such as $1 - 1 + 1 - 1 + \cdots$ are divergent because, although the partial sums don't become infinite, they do not approach a unique limit as n increases beyond all limit. Further, Stokes defined the difference between conditional and absolute convergence and gave an example. Stokes called absolutely convergent series *essentially convergent* and conditionally convergent series *accidentally convergent*.

Stokes then made a statement about how and why divergent series may be used. A divergent series may be used when the first n terms of the divergent series can be seen as some type of limit of a convergent series. Stokes said:

> "Of course the first n terms of a divergent series may be the limits of those of a convergent series: nor does it appear possible to invent a series so rapidly divergent that it shall not be possible to find a convergent series which shall have for the limits of its first n terms the first n terms respectively of the divergent series. Of course we may employ a divergent series merely as an abbreviated mode of expressing the limit of the sum of a convergent series. Whenever a divergent series is employed in this way in the present paper, it will be expressly stated that the series is so regarded." (Stokes 1849, p.536)

Later in the paper, Stokes repeated himself and addressed the issue of whether a divergent series could have a different sum depending on which convergent series it was considered a limit of. Stokes said:

> "I would here make one remark on the subject of consistency. We may speak of the sum of an infinite series which is not convergent, if we define it to mean the limit of the sum of a convergent series of which the first n terms become in the limit the same as those of the divergent series. According to this definition, it appears quite conceivable that the same divergent series should have a different sum according as it is regarded as the limit of one convergent series or of another. If however we are careful in the same investigation always to regard the same divergent series, and the series derived from it, as the limits of the same convergent series and the series derived from it, it does not appear possible to fall into error, assuming of course that we always reason correctly. For example,

we may employ the series [...], and the series derived from it by differentiation, &c., without fear, provided we always regard these series when divergent, or only accidentally convergent, as the limits of the *particular* convergent series formed by multiplying their n^{th} term by g^n." (Stokes 1849, p.539)

These long quotes indicate what Stokes meant, in 1847, by a divergent series. He articulated when it was safe to use them and how they could be used. Stokes' statements are lacking in rigour and, in fact, are difficult to understand. For example, what does it mean to "assume that we always reason correctly"? Stokes overcame the lack of clarity about how to ensure the veracity of results obtained from an asymptotic expansion in two ways.

First, Stokes compared his asymptotic expansion results to results obtained from theory by other methods — computation from a convergent series, or by evaluation of a definite integral in finite terms, for example. In this vein, Stokes footnoted a paper *On Fluctuating Functions* (Hamilton 1843, p.264) written by Sir Rowan William Hamilton (1805-1865) which he also referred to in the paper *On the Numerical Calculation of a Class of Definite Integral*, analyzed next. Stokes referred to this paper by Hamilton twice because it justified the results he obtained asymptotically via a completely different method. Second, Stokes compared his asymptotic results to experimental evidence.

The key point is that the mathematics of Stokes' asymptotic method did not stand on its own. By way of contrast, 55 years later, a standard textbook, *A Course of Modern Analysis* (Whittaker 1902) by Edmund Taylor Whittaker (1873-1956) published in 1902, which we will say more about later, defined an asymptotic expansion thus:

"A divergent series

$$A_0 + \frac{A_1}{x} + \frac{A_2}{x^2} + \cdots + \frac{A_n}{x^n} + \cdots$$

in which the sum of the first $(n+1)$ terms is S_n is said to be an *asymptotic expansion* of a function $f(x)$, if the expression $x^n\{f(x) - S_n\}$ tends to zero as x (supposed for the present to be real and positive) increases indefinitely." (Whittaker 1902, p.164)

Whittaker, roughly fifty years after Stokes' paper, started with a clear definition of an asymptotic expansion. This is unlike how Stokes presented his thoughts on asymptotic expansions.

Finally, Stokes showed that if a function had an expansion into a convergent series of sine functions, it was a simple matter to find what the coefficients were. He then addressed several issues in detail regarding the use of Fourier series. These included:

1. Finding discontinuities of Fourier series.

2. Finding discontinuities of the integrals that are analogous to the Fourier series.

3. Finding discontinuities of sums of infinite series and improper integrals.

I now look at how Stokes found asymptotic expansions.

3.2 Stokes' 1850 paper on Numerical Calculation of Integrals

I have shown that in the mid-nineteenth century the validity of using divergent series was not agreed upon and yet there was awareness that they could be very useful if used cautiously. This position was clearly seen in the De Morgan paper. The questions then are: when should they be used?, how do you come up with them?, what justification is used for employing them?, and how are any results thus obtained verified?

Further, we have seen that Stokes, in 1847, published a paper where convergence issues were carefully examined — a paper in which he defined what he meant by convergence and divergence and a paper in which the idea of uniform convergence was nascent.

Then, Stokes, at mid-century (March 11, 1850), used divergent series to obtain numerical approximations for several definite integrals arising in optics. He then used the same method in computing the vacuum to air correction factors from pendulum theory (December 9, 1850). I will use the computation of the value of a definite integral that arose in optics to illustrate Stokes' use of a divergent series in a particular context.

Stokes first used his divergent series method to obtain numerical results in optical theory and it was then very shortly afterwards used in pendulum theory. The publication dates indicate this but Stokes himself stated that after developing his technique to evaluate a definite integral brought to his attention by Airy, he realized that the same method could be used to compute the vacuum to air correction factor in the case of the cylindrical pendulum — a problem that he had earlier set aside when the computations appeared to be too difficult.

Stokes was interested in calculating the numerical values of the definite integral

$$W = \int_0^\infty \cos \frac{\pi}{2}(w^3 - mw)\ dw$$

for large values of the parameter m. Large in this context meant larger than 4, since Airy had already done the computations up to that value.

This integral, called the Airy integral, arose while trying to determine the intensity of light in the neighbourhood of a caustic. A caustic is the envelope of light rays reflected or refracted by a curved surface. The light intensity an any particular location in the caustic was mathematically determined using the undulatory theory of light. This theory was formulated by Augustin-Jean Fresnel (1788-1827) and published in a series of memoirs during the late 1810s and early 1820s culminating in *De la lumière* published in 1822. This work was translated into English by Young between 1827 and 1829 and fully accepted by Airy by 1831 (Buchwald 1989).

In the early part of the nineteenth century, there was vigorous debate about what light was. Many of the well-established scientists of this time were followers of Newton's corpuscular theory of light which held that light was an emission of particles from a luminous object. The competing theory was the wave theory of light which held that light was a vibration in some type of ether — a hypothetical medium that filled space and through which light travelled. Various experiments were done in order to try to understand which theory best explained observed phenomena. These experiments included observations of refraction, interference, diffraction and polarization.

Proponents of these competing theories each tried to use their theory to explain experimental results. In 1817 the French Académie des Sciences set diffraction as its prize competition topic. In response to this and building on it afterwards, Fresnel proposed a wave theory of light where light was vibration in an ether and further, that the movement of light through the ether was a result of transverse vibration. For a history of optics during this time, see *A History of Optics from Greek Antiquity to the Nineteenth Century* by Olivier Darrigol (Darrigol 2012). For a detailed explanation of the rise of the wave theory of light see *The Rise of the Wave Theory of Light: Optical Theory and Experiment in the Early Nineteenth Century* by Jed Buchwald (Buchwald 1989).

Airy's paper *On the Intensity of Light in the Neighbourhood of a Caustic* (Airy 1838) appeared in Volume 6 of the *Transactions of the Cambridge Philosophical Society* in 1838. In the first paragraph of this paper Airy explicitly stated the motivation for his work. He sought to generate predictions from the undulatory theory of light to which he could compare with the results from experiments performed with the intent of testing the theory. Airy stated it thus:

> "When a great physical theory has been established originally on considerations and experiments of a simple kind, which by degrees have been exchanged for

> comparisons of more distant results of the theory with more complicated cases of experiment, it has always been considered a matter of great interest, to trace out accurately by mathematical process the consequences, according to that theory, of different modifications of circumstances: which can then be compared with measures that have been made, or that may easily be made in future. It is with this view that I solicit the indulgence of the Society, for the following investigation of the Intensity of Light in the neighbourhood of a Caustic, as mathematically estimated from the Undulatory Theory." (Airy 1838, p.379)

Here, analogous to Stokes and the pendulum measurements, Airy had a theory which he wanted to verify by using it to make predictions which he could then test in the laboratory. After many pages of analysis, Airy concluded that the expression for the disturbance of the ether at the illuminated point, which is related the intensity of light in the neighbourhood of the caustic, was

$$\sin \frac{2\pi}{\lambda}(vt - E) \int_0^\infty \cos \frac{\pi}{2}(w^3 - mw)\, dw$$

and therefore the intensity of light was

$$\left[\int_0^\infty \cos \frac{\pi}{2}(w^3 - mw)\, dw\right]^2$$

Airy computed the value of the integral to obtain the light intensity for values of $m = -4.0$ to $m = 4.0$ in steps of 0.2 which he published in a table in the paper (Airy 1838, p.391). The method that Airy used for computing these integral values was a modified midpoint rule:

1. First the upper limit of the integral was reduced from $w = \infty$ to $w = 2$.

2. Then the integral was divided into 8 pieces, corresponding nearly to quadrants of the circular function when $m = 0$. This gave the following 8 integrals $w = 0$ to $w = 1.00$, $w = 1.00$ to $w = 1.26$, $w = 1.26$ to $w = 1.44$, $w = 1.44$ to $w = 1.58$, $w = 1.58$ to $w = 1.70$, $w = 1.70$ to $w = 1.82$, $w = 1.82$ to $w = 1.92$ and $w = 1.92$ to $w = 2.00$.

3. For each of the integrals, a mesh size that ranged from 0.04 for the first integral to 0.008 for the last integral was selected.

4. For each of these integrals, the midpoint rule was used to approximate the integral.

5. The first four differences were computed for each interval of integration and the midpoint rule was improved in effect fitting a quartic polynomial to each region.

The method of numerical quadrature that Airy used to directly evaluate the integral was a standard technique at that time. For a thorough analysis of the typical numerical methods used for quadrature during the nineteenth century, see Herman Goldstine's book titled *A History of Numerical Analysis from the* 16^{th} *through* 19^{th} *Century* (Goldstine 1977).

Airy further computed W, the square root of the light intensity, for $w = 2.00$ to $w = \infty$ using substitution and integration by parts. The integration by parts was performed seven times and then the remainder, a residual integral, beyond that was discarded. This method produced a partial sum in the form of an alternating divergent series.

Airy was well aware that the method he employed, with the lower limit of integration of two for

$$W = \int_2^\infty \cos \frac{\pi}{2}(w^3 - mw)\ dw$$

produced a divergent series. Regarding the error incurred by using an alternating divergent series, Airy said:

> "The residual integral, therefore, is certainly less than v_n, the last term found in the series, and is probably much less: and therefore, if the last term computed consist only of integers in the last place of decimals which we wish to retain, even though the divergence of the series be just beginning, the use of these terms will give the integral required with the utmost practical accuracy." (Airy 1838, p.400)

This bound on the error of the partial sum of the series is the alternating series test error bound which had been well-known for a long time.

For values of m approaching $m = 3.0$, the divergence in the series Airy produced commenced sooner. He corrected for that as well stating that the number of terms computed by a logarithmic process for the part of the integral beginning at 2 was 900 for values of m that were large (above 3.0). Airy estimated that the error that remained in these calculations were to digits in the fifth place of decimals, but no higher. Computations were impractical with the above method beyond $m = 4.0$.

Later, Airy was able to recompute these values and to include values of the intensity integral (now regularly referred to as the Airy integral) for values of m between ± 5.6 using a convergent series expansion of the integrand and then integrating term by term. Though this was an improvement, the calculations beyond $m = \pm 5.6$ remained too laborious and the value of the integral for larger values of m remained unknown.

The values of m for which $W = 0$ give physical information about the intensity of light in the neighbourhood of a caustic — specifically $W = 0$ when there is a dark band in the illumination. The values of the integral computed by Airy made it possible to find the first two roots of W. However, Miller had observed the dark bands in the caustic in the laboratory and had thus experimentally determined the first 30 zeros of the function W. The problem for Stokes was to compute the locations of those dark bands using the integral.

Several things about this problem command our attention. The divergent series that Stokes developed to make these numeric computations came from a convergent integral so the existence of a finite value was guaranteed. In addition there were already two methods of computing the integral for restricted values of m, one of which did not employ a divergent series and the other which employed an alternating divergent series for a portion of the integral — a portion which was significant for some values of m and insignificant for others.

Further, there was experimental evidence against which the mathematical calculations could be compared. This was Airy's motivation for making the calculations in the first place. According to Stokes, after some effort, he was able to manipulate the Airy integral into a divergent series from which he easily computed W for large values of m. He was also able to find the values of m which made $W = 0$. Stokes said:

> "After many trials I at last succeeded in putting Mr. Airy's integral under a form from which its numerical value can be calculated with extreme facility when m is large, whether positive or negative, or even moderately large. Moreover the form of the expression points out, without any numerical expression, the law of the progress of the function when m is large. It is very easy to deduce from this expression a formula which gives the i^{th} root of the equation $W = 0$ with hardly any numerical calculation, except what arises from merely passing from $(m/3)^{\frac{3}{2}}$, the quantity given immediately, to m itself." (Stokes 1856b, p.167)

Before describing his new method for computation, Stokes commented on its usefulness. The integral W had already been developed in an ascending convergent power series. According to Stokes, ascending power series are effectively Taylor series where in each subsequent term the variable is raised to a larger positive power and these types of ascending series occur constantly in solutions to physical problems. Examples of such series, provided by Stokes, which are convergent for all values of the variable, are e^{-x}, $\cos x$ and $\sin x$ all of which are easily computed, with high accuracy, for small values of x but are extremely inconvenient to use for large values of x because too many terms must be computed in order to get sufficient accuracy. Note here that all three examples Stokes gave are alternating series. He did not use e^x as an example even though his statement is still true for that function even though the representative infinite series does not alternate. Perhaps these examples were chosen because there is an obvious error bound in the first omitted term. On the other hand, the Taylor remainder theorem was available to Stokes both in the Lagrange and Cauchy form though neither of these is as simple to use as the alternating series test.

Stokes was motivated to use divergent series in the first place precisely because convergent ascending series, like those above, are computationally inefficient. Stokes also sought to find what he termed the "law of progress" of a function. Stokes used this expression to mean to determine how a function changes as the variable changes and specifically to understand function behaviour as the variable increases. For example, consider $f(x) = \sin x$. A visual picture of this is easily formed and the behaviour of the function for large x can be seen. Representing $f(x) = \sin x = x - \frac{x^3}{3!} + \frac{x^5}{5!} - \cdots$ in its ascending power series makes the function much harder to visualize. It is also more difficult to figure out what the limit of the function is as the variable tends to infinity or to find the zeros of the function.

According to Stokes, such series can present themselves as a development of a definite integral arising in the solution of a physical problem, as in the integral for W above. This results from computing the Taylor series of the integrand and then integrating term by term. Convergent infinite series can also present themselves as the development of integrals which arise as solutions to a linear differential equation which cannot be integrated in finite terms. Stokes claimed his method was very generally applicable to series of this second type.

Stokes claimed his method was difficult to describe in general and was best understood from examples, of which he provided three. The first example was the Airy integral

$$W = \int_2^\infty \cos \frac{\pi}{2}(w^3 - mw)\ dw$$

with parameter m such that $W = W(m)$. The second example was

$$u = \frac{2}{\pi}\int_0^{\frac{\pi}{2}} \cos(x\cos\theta)\ d\theta$$

with parameter x such that $u = u(x)$, and the third example was

$$v = \frac{2}{\pi}\int_0^x \int_0^{\frac{\pi}{2}} \cos(x\cos\theta)\ x\ dx\ d\theta.$$

where the upper limit of integration of the first integral is a parameter in the second integral such that $v = v(x)$. The second example arose, according to Stokes, in a great many physical applications, and the third example occurred when investigating diffraction with a circular aperture in front of a lens.

Despite the fact that Stokes claimed that it was difficult to describe his method in general, he gave an overview of the method which I summarize before I describe in the method in detail using the second of his three examples. Consider a series of the type described above — that is, an ascending infinite series resulting from an integral solution to a physical problem or appearing in the solution to a linear differential equation. For infinite series of this type Stokes' method to find an asymptotic expansion was as follows:

1. Identify the differential equation which is solved by the given series $y = f(x)$ where $f(x)$ is a convergent infinite series.

2. Determine which terms of the differential equation are important when x is large, eliminate the other terms in the differential equation and solve the differential equation in finite terms.

3. Assume that the solution to the original differential equation is the, typically circular or exponential, function found in part 2) multiplied by an infinite series in descending powers of x. This descending series has undetermined coefficients.

4. Use either the original integral (easier) or use the original convergent series (more difficult) to find the undetermined coefficients introduced in step 3).

Step 3 of the method above was suggested to Stokes after he saw the formulae Cauchy developed for computation of the Fresnel integrals for large values of the upper limit of integration. Stokes saw these in *Répertoire d'optique moderne* (Moigno 1850), a work written by François-Napoléon-Marie Moigno (1804-1884) between 1847 and 1850. Moigno considered himself a student of Cauchy. As we saw earlier in the introduction, Cauchy was opposed, in the main, to the use of divergent series, yet he wrote a paper on their legitimate use. And, as reported by Moigno, Cauchy produced a divergent series representation of the Fresnel integrals by repeated use of integration by parts. He then used the partial sums of those divergent series to evaluate the Fresnel integrals for large values of the upper limit of integration.

To illustrate Stokes' method, I have chosen to use the second of his three examples since the computations are easiest to follow in that case. Consider the integral

$$u = \frac{2}{\pi} \int_0^{\frac{\pi}{2}} \cos(x \cos\theta)\ d\theta$$

where x is a parameter such that $u = u(x)$. Take the integrand of u and expand the outer cosine function in a Taylor series centered at zero to get

$$u = \frac{2}{\pi} \int_0^{\frac{\pi}{2}} (1 - \frac{x^2\cos^2(\theta)}{2!} + \frac{x^4\cos^4(\theta)}{4!} - \frac{x^6\cos^6(\theta)}{6!} + \cdots)\ d\theta.$$

Then integrate term by term to get a convergent series with interval of convergence $x \in (-\infty, \infty)$ in ascending powers of x:

$$u = 1 - \frac{x^2}{2^2} + \frac{x^4}{2^2 4^2} - \frac{x^6}{2^2 4^2 6^2} + ...$$

This is this Bessel function of the first kind of order zero — a known series. When developing this infinite series from the integral, the order of summation and integration were switched without comment. This is typical in this paper — Stokes did not discuss convergence issues while explaining his method.

For small values of x, successive terms in the series for $u(x)$ get very small very quickly, and a good estimate for $u(x)$ can be obtained by truncating the series after a small number of terms. For larger values of x, however, the terms grow for a considerable period of time before decreasing and many terms must be included in order for the error, which is bounded by the first term neglected since $u(x)$ is an alternating series, to become insignificant. It is this fact that makes the manual computations of such definite integrals unmanageable for large values of the parameter which in this case is x.

The values of the integral u had been already tabulated by Airy and published in a table in the eighteenth volume of the *Philosophical Magazine* (Airy 1841, p.1) for values of x from zero to ten, at intervals of 0.2. This gave Stokes something to check his computations, and thus his new method, against.

The next step Stokes took in finding a divergent series representation of u was to find a differential equation which was satisfied by the power series representation of u. This was done by performing the operation

$$x\frac{d}{dx}$$

twice on the series u and noticing that

$$-x^2u$$

was the result. There was no algorithm for this — you differentiate twice and multiply by powers of x and see if you can determine an equation. Thus Stokes found that u satisfied the differential equation

$$\frac{d^2u}{dx^2} + \frac{1}{x}\frac{du}{dx} + u = 0$$

Stokes next made two arguments for only considering the first and last term of the above differential equation for large x. The first argument, presented in the main body of the paper, was that an increment in x, which Stokes labelled δx, while perhaps not small itself was small in relation to x when x was very large. Thus, if the increment in x was equated with $\frac{du}{dx}$, the second term of the differential equation was small when compared to the first and third terms.

In a footnote, Stokes made a second argument for omitting the second term of the differential equation. This argument consisted of taking all possible combinations of two terms of the differential equation and showing that the solution of the differential equation under any combination but omitting the second term, results in a function whereby the omitted term was more important than the retained terms.

Stokes said this as follows:

> "That the 1st and 3rd terms in $\left[\frac{d^2u}{dx^2} + \frac{1}{x}\frac{du}{dx} + u = 0\right]$ are ultimately the important terms, may readily be seen by trying the terms two and two in the way mentioned in the introduction. Thus, if we suppose the first two to be the important terms, we get ultimately $U = A$ or $U = B \log x$, either of which would render the last term more important than the 1st or 2nd, and if we suppose the 2nd and 3rd to be the important terms, we get ultimately $u = Ae^{\frac{-x^2}{2}}$, which would render the first term more important than either of the others." (Stokes 1856b, p.182)

In the quote above when Stokes referred to the way mentioned in the introduction for trying terms two by two, he said that this was "much as in Lagrange's method of expanding implicit functions which is given by Lacroix in his *Traité du Calcul*" (Stokes 1856b, p.167).

There is no comparison between retaining the first and third term and using all three terms presumably because the differential equation can be solved in finite terms by standard methods only when two of the three terms are included. Thus the form of the solution can only be seen for various values of x for all possible cases with one omitted term.

The simplified differential equation, with the second term omitted, now called the Van der Pol equation is

$$\frac{d^2u}{dx^2} + u = 0$$

and is solved, as per Stokes, with the function

$$u = A \cos \delta x + B \sin \delta x.$$

It is not clear here why Stokes used δx here rather than x since the solution to the Van der Pol equation is:

$$u = A\cos x + B\sin x.$$

The δx notation was not used further in Stokes' analysis; however this notation was used in contemporary work.

The presence of the circular functions in the solution to the simplified differential equation led Stokes to assume a solution to the original differential equation with all three terms to be of the form

$$u = e^{x\sqrt{-1}}(Ax^{\alpha} + Bx^{\beta} + Cx^{\gamma} + ...)$$

where the values of A, B, C, ... and α, β, γ, ... were undetermined. I am using the exact notation of Stokes and I note that the A and B in the full solution do not represent the same thing as in the equation $u = A\cos\delta x + B\sin\delta x$ which is the solution to the simplified differential equation.

This solution to the original differential equation can be seen as one where the solution to the differential equation of two terms (a linear combination of the circular functions) has been factored out.

The proposed solution, $u = e^{x\sqrt{-1}}(Ax^{\alpha} + Bx^{\beta} + Cx^{\gamma} + ...)$, and its first and second derivatives were substituted back into the complete differential equation to provide the following equation:

$$\sqrt{-1}\{(2\alpha+1)Ax^{\alpha-1} + (2\beta+1)Bx^{\beta-1} + \cdots\} + \alpha^2 Ax^{\alpha-2} + \beta^2 Bx^{\beta-2} + \cdots = 0$$

which, under the assumption that the desired series solution be in descending powers of x forced

$$\alpha = -\frac{1}{2},\ \beta = -\frac{3}{2},\ \gamma = -\frac{5}{2}, \cdots$$

By equating the coefficients on like powers of x, Stokes determined the values of all of the constants A, B, C, ... in terms of A. After substituting for B, C, ... into the solution, it was then possible to algebraically manipulate the solution into the form

$$A(P + \sqrt{-1}Q).$$

The above procedure was repeated to give a second solution of the form

$$B(P - \sqrt{-1}Q)$$

and the combination of these two solutions gave the general solution to the original differential equation of

$$u = Ax^{-\frac{1}{2}}(R\cos x + S\sin x) + Bx^{-\frac{1}{2}}(R\sin x - S\cos x)$$

where

$$R = 1 - \frac{1^2 \cdot 3^2}{1 \cdot 2(8x)^2} + \frac{1^2 \cdot 3^2 \cdot 5^2 \cdot 7^2}{1 \cdot 2 \cdot 3 \cdot 4(8x)^4} \cdots$$

and

$$S = \frac{1^2}{1 \cdot (8x)} - \frac{1^2 \cdot 3^2 \cdot 5^2}{1 \cdot 2 \cdot 3(8x)^3} \cdots$$

This new A and B here were determined from initial conditions which were obtained from the original equation in the following manner. Consider the original integral to be evaluated:

$$u = \frac{2}{\pi}\int_0^{\frac{\pi}{2}} \cos(x\cos\theta)\ d\theta = u = 1 - \frac{x^2}{2^2} + \frac{x^4}{2^2 4^2} - \frac{x^6}{2^2 4^2 6^2} + ...$$

and substitute $\cos\theta = 1 - \mu$ into the integral. This substitution, for large values of x, yielded an approximation to the integral by the function

$$u = (\pi x)^{-\frac{1}{2}}(\cos x + \sin x)$$

By comparing this solution, which came directly from the convergent integral using simplifying assumptions based on large x, with

$$u = Ax^{-\frac{1}{2}}(R\cos x + S\sin x) + Bx^{-\frac{1}{2}}(R\sin x - S\cos x)$$

which came from the solution to the differential equation in terms of a divergent series, Stokes was able to determine the constants. This yielded a value of $\pi^{-\frac{1}{2}}$ for both A and B.

Ultimately the result was that for very large values of x, u can be computed via

$$u = \left(\frac{2}{\pi x}\right)^{\frac{1}{2}} R\cos(x - \frac{\pi}{4}) + \left(\frac{2}{\pi x}\right)^{\frac{1}{2}} S\sin(x - \frac{\pi}{4})$$

where R and S are divergent series in descending powers of x.

At this point, Stokes lent authority to his result by noting that Hamilton had already obtained this result using a different method. Stokes said:

> "This expression for u, or rather an expression differing from it in nothing but notation and arrangement, has been already obtained in a different manner by Sir William R. Hamilton in a memoir *On Fluctuating Functions. See Transactions of the Royal Irish Academy, Vol. XIX, p.313*." (Stokes 1856b, p.182)

This was the first justification of the validity of the method outlined. There was confirmation of this directly from Hamilton but it came about 7 years later. On December 17th, 1857, Hamilton wrote to Stokes that he had received the 1850 paper by post nearly a month prior. Hamilton said that he had received a copy earlier but that he was otherwise occupied and that he was pleased to receive a second copy of the important paper. Hamilton was pleased to be credited with finding the asymptotic expansion for the Bessel function of order zero by a different method but with the same result of Stokes using his "quite different method" (Stokes 1907b, p.131). Hamilton stated that:

> "These *numbers* (as I had the satisfaction to observe) agreeing with those of your formula (60), but the *law* having been independently deduced." (Stokes 1907b, p.131)

Immediately after finding his asymptotic expansion, Stokes computed the value of u for $x = 10$ and obtained $u = -0.24594$ which Airy had computed to be -0.2460 from the convergent series. In order to compare his results with Airy's result, Stokes picked the largest value of x for which a numerical result was known. It is not clear what Stokes considers to be a large value of x since the word large was not quantified in the paper. However, it made sense to verify the method with the largest value of x for which computation via a different method was available. This was the second justification for the method outlined.

Stokes continued his analysis of his asymptotic solution, $u(x)$, and showed how to convert the solution $u(x)$ into a formula for finding the roots of $u(x)$. From this formula, all of the roots of $u(x)$ with the exception of the first were found. The value of those roots, which determine the dark bands in the caustic, agreed with the computations of Airy which had previously been done using a convergent series. This was the third justification for the method outlined.

3.3 Use and Reception of the Stokes Method

Stokes read the paper that explained in detail how he was able to use divergent series for numerical approximation in the spring of 1850. However he was using this method at least as early at 1848.

Stokes wrote a short letter to Airy (Stokes 1907a, p.159) on May 12th, 1848. The purpose of the letter was to respond to an enquiry of Airy, communicated to Stokes by Miller, about numerical approximations of the Airy integral. These were the calculations that led to the determination of the null-points of Airy's integral. In this letter, Stokes immediately stated the divergent series representation of the integral (worked out in example one in the 1850 paper *On the numerical Calculation of a Class of Definite Integrals and Infinite Series.*), said that it diverged "hyper-geometrically" for any value of m or x but that for moderately large or large values of m, the leading terms converged with "great rapidity" and the first few terms gave a very good approximation.

In this letter, Stokes does not appear completely confident in the validity of his method. Just prior to telling Airy the values of the zeros of the Airy integral he said:

> "The series shows (assuming that the convergent part is really an approximation to the required integral) that w vanishes for..." (Stokes 1907a, p.159)

It was not uncommon at this time to use the terminology, convergent part, to mean the terms of the divergent series which were decreasing in magnitude. The diverging part of the series, then, was the terms that followed the smallest term of the divergent series.

Shortly after that Stokes carefully pointed out that his divergent series approximation failed for small values of x and m. However, Airy's calculations showed that there was no dark band until m was large enough to make Stokes' approximation valid. Stokes also pointed out the utility of his method stating that he calculated the first fifteen vanishing points of the integral in less than one and a half hours.

He further confessed that he had been unable to bound the error. Stokes said:

> "I have not as yet assigned a limit to the error committed in stopping at any term, but I hope to be able to do so. I am however at present occupied with other investigations." (Stokes 1907a, p.160)

Does this perhaps indicate that Stokes did not feel it tremendously important to provide an error bound and was content to rely on the comparison of his results to those obtained by other methods?

Stokes also stated that the method worked for an integral that Airy had tabulated in *Philosophical Magazine* of January 1841 and that he expected it to work for the integral that applied in the case of circular hole in front of the object-glass (lens) of a telescope which was Example 3 of the 1850 paper, written two years later. Further Stokes communicated that he believed his method would apply to a variety of integrals of that type (Stokes 1907a, p.160).

The extensive correspondence between Stokes and Airy is 34 pages of Volume 2 of Stokes' correspondence (Stokes 1907a, p.159-192). The first letter of their correspondence is the May 12, 1848 letter quoted above. Either Airy did not reply to this letter or perhaps the reply was lost — whatever the reason, the collected correspondence does not contain a reply from Airy.

I have shown that Stokes' method for asymptotic approximation was developed in the late 1840s. Stokes was carefully considering convergence at the end of 1847. By early 1848, as communicated to Airy, Stokes had an asymptotic expansion method that was somewhat general and had been applied to the Airy integral. His paper on his asymptotic expansion method was read in 1850 and published in the *Transactions* in 1856. The 1857 letter from Hamilton to Stokes indicates that Stokes' method of 1848 was circulated by post in 1850.

The caution regarding his method that Stokes displayed when writing to Airy in 1848 is not present in the paper of 1850. There was also complete lack of discussion of convergence issues in the 1850 paper, notable particularly given the very careful analysis of convergence three years earlier.

As a testament to the importance of Stokes' work analyzed here and, in particular, the paper *On the Numerical Calculation of a Class of Definite Integrals and Infinite Series*, I quote from Lord Kelvin's tribute to Stokes written on February 12, 1902, ten days after Stokes' death. Kelvin said:

> "Even pure mathematics of a highly transcendental kind has been enriched by his penetrating genius; witness his paper 'On the Numerical Calculation of a Class of Definite Integrals and Infinite Series*,' called forth by Airy's admirable paper on the intensity of light in the neighbourhood of a caustic, practically the theory of the rainbow. Prof. Miller had succeeded in observing thirty out of an endless series of dark bands in a series of spurious rainbows, for the determination of which Airy had given a transcendental equation and had calculated, of necessity most laboriously by aid of ten-figure logarithms, results giving only two of those black bands. Stokes, by mathematical supersubtlety, transformed Airy's integral into a form by which the light at any point of any of those thirty bands, and any

> desired greater number of them, could be calculated with but little labour, and with greater and greater ease for the more and more distant places where Airy's direct formula became more and more impracticably laborious. He actually calculated fifty of the roots, giving the positions of twenty black bands beyond the thirty seen by Miller." (Stokes 1907b, p.307)

Thus, this work of Stokes was recognized much later as being a highlight of his early career. I claim that Stokes was first to develop this type of method to produce an asymptotic approximation. This is also the opinion of Schlissel who said:

> "Stokes was the first investigator to use the complete formal solution as an approximation tool, in contrast to Carlini who had retained only the first two terms. He was also the first who seriously attempted to prove that the approximate solution in some sense approximated an actual solution. However Stokes gave no estimate for the error incurred when only part of the approximating series is used, and the argument he used to establish a relation between the approximating series and the actual solution is generally untrue. A method developed later by Poincaré did demonstrate that the series of the type obtained by Stokes, later to be called normal series solutions, were indeed the asymptotic series solutions of a general class of differential equations." (Schlissel 1977, p.318)

An obvious question to ask is what happened to this new tool in the period of time just following 1850, when at least Airy was aware of it, or even following 1856, when it was widely available to the scientific community after publication in the *Transactions.* The answer to that question appears to be very little. The results that Stokes provided to Airy were seen to be important but that does not seem to have translated into an uptake in the use of the Stokes approximation method by others or even by Stokes himself in other contexts.

There is a paper, read in 1851, and published in the 1856 volume of the *Transactions* by J.H Rohrs titled *On the Oscillations of a Suspension Chain* (Rohrs 1856, p.379-398) concerning the important topic of how vibrations could lead to bridge collapse. In this paper Rohrs refers explicitly to Stokes' paper *On the critical values of sums of periodic series* and he also refers several times to assistance he got from Stokes in order to make certain calculations. It is possible the the asymptotic method was used to make those calculations but that is not clearly displayed in this paper.

Stokes published again on the topic of divergent series in Volume 10 (1864) of the *Transactions* in a paper titled *On the Discontinuity of Arbitrary Constants which appear in Divergent Developments* (Stokes 1864, p.105-124) which was read May 11, 1857. This paper starts with a reference to *On the Numerical Calculation of a Class of Definite Integrals and Infinite Series* and a reminder that Stokes had found a development of the Airy integral that admitted extremely easy numerical calculation.

Stokes began *On the Discontinuity of Arbitrary Constants which appear in Divergent Developments* with a statement of the value of his method. He said:

> "The method there followed is of very general application to a class of functions which frequently occur in physical problems. Some other examples of its use are given in the same paper; and I was enabled by the application of it to solve the problem of the motion of the fluid surrounding a pendulum of the form of a long cylinder, when the internal friction of the fluid is taken into account." (Stokes 1864, p.105)

Stokes considered this paper, read in 1857, as a supplement to his 1850 paper. The application to the cylindrical pendulum bob quoted above was exactly the application he had already discussed in 1850. Stokes, seven years later, does not indicate that he has solved any further problems with his method.

Stokes further indicated the value of his method for calculation. He said:

> "In my former paper, to which the present may be regarded as a supplement, I have employed these equations to obtain integrals in the form of descending series multiplied by exponentials. These integrals, when once the arbitrary constants are determined, are exceedingly convenient for numerical calculation when the variable is large, notwithstanding that the series involved in them, though at first rapidly convergent, became ultimately rapidly divergent." (Stokes 1864, p.105)

The main purpose of the 1857 paper was to discuss the arbitrary constants obtained when determining the asymptotic expansion. Stokes indicated that it was possible to find these in two different ways — numerically or analytically.

Consider two different infinite series representations of a function of interest. One series consists of ascending terms — this is a convergent series — and the other series consists of descending terms — this is a divergent series. There are constants that must be determined that are introduced during the development of the divergent series expansion.

These constants can be found numerically by computing the value of the convergent and the divergent series for one or more values of the variable and then equating the results to find the constants. This method had the advantage of being generally applicable but, according to Stokes, “wholly devoid of elegance” (Stokes 1864, p.105).

The analytic method consisted of finding a relationship between the constants in the ascending and descending series by means of a definite integral. With one exception, this was what Stokes did in the previous work of 1850. In the case of the Airy integral Stokes was able to find constants analytically for values of m positive, but he was not able to do so for values of m negative. He did find the values of the constants for m negative but was unable to give a “satisfactory demonstration of it” (Stokes 1864, p.106).

The reason for this is what is now called Stokes’ phenomena and Stokes is stating that the value of the constants in his asymptotic expansions are different for different values of the argument of the variable and further, that these constants change in a discontinuous manner. He appears to have noticed it first when he changed the parameter in the Airy integral expansion from positive and real to negative and real — that is, the asymptotic expansion was different for different classes of the variable. The value of constants exhibits discontinuities as the argument of m changed from zero to π.

In 1857, he further considered his asymptotic expansions for complex values of the argument and he wanted to understand exactly where the discontinuity occurred and how the constants changed. This would allow the divergent series to be used for all complex values of the variable. The place where these discontinuities occur in the complex plane are now called Stokes’ lines. Stokes said:

> “But though the arbitrary constants which occur as coefficients of the divergent series may be completely determined for real values of the variable, or even for imaginary values with their amplitudes lying between restricted limits, something yet remains to be done in order to render the expression by means of divergent series analytically perfect. I have already remarked in the former paper (p. 176) that inasmuch as the descending series contain radicals which do not appear in the ascending series, we may see, a priori, that the arbitrary constants must be discontinuous. But it is not enough to know that they must be discontinuous; we must also know where the discontinuity takes place, and to what the constants change. Then, and not till then, will the expressions by descending series be complete, inasmuch as we shall be able to use them for all values of the amplitude of the variable.” (Stokes 1864, p.106)

Stokes considered his 1857 paper a resolution to the stated problem. He said:

"I have now succeeded in ascertaining the character by which the liability to discontinuity in these arbitrary constants may be ascertained. I may mention at once that it consists in this; that an associated divergent series comes to have all its terms regularly positive. The expression becomes thereby *to a certain extent illusory*; and thus it is that analysis gets over the apparent paradox of furnishing a discontinuous expression for a continuous function. It will be found that the expressions by divergent series will thus acquire all the requisite generality, and that though applied without any restriction as to the amplitude of the variable they will contain only as many unknown constants as correspond to the degree of the differential equation. The determination, among other things, of the constants in the development of Mr Airy's integral will thus be rendered complete." (Stokes 1864, p.106)

Stokes, in 1857, proceeded in exactly the same manner as in 1850 and used a simple example in order to introduce what followed. The example was different function from the 1850 paper function but again Stokes started with an integral and its related ascending series both of which were convergent for all values of the variable, real or imaginary:

$$u = 2\int_0^\infty e^{-x^3} \sin 2ax\ dx = \frac{2a}{1} - \frac{(2a)^3}{2.3} + \frac{(2a)^5}{3.4.5} \cdots$$

The development of the asymptotic expansion to be used to compute the value of u for large values of a followed which included a lengthy discussion about the validity of using the asymptotic expansion, including how the expansion could be used and for which values of the argument the expansion was valid.

Stokes then took the Airy integral and its expansion from the 1850 paper and explained how the constants in the asymptotic expansion must be discontinuous. Further, Stokes explained there was a constant in the expansion of the integral regarding the determination of the resultant force of the fluid on the pendulum that was left undetermined. This constant was determined in the 1857 paper and was necessary for determining the motion of the fluid at a great distance from the pendulum (Stokes 1864, p.122).

Stokes had previously noted that the constants in the asymptotic expansion of the Airy integral were different for positive and negative real values of the variable. In this later paper, he stated that the constants in his asymptotic expansions changed discontinuously as the imaginary part of the (now complex) variable changed. Stokes concluded the following:

"That when functions expressible in convergent series according to ascending powers of the variable are transformed so as to be expressed by exponentials

> multiplied by series according to descending powers, applicable to the calculation of the functions for large values of the variable, and ultimately divergent, though at first rapidly convergent, the series contain in general discontinuous constants, which change abruptly as the amplitude of the imaginary variable passes through certain values." (Stokes 1864, p.124)

Stokes wrote to his soon to be wife in March of 1857 about this. He said:

> "Some years ago I attacked an integral of Airy's, and after a severe trial reduced it to a readily calculable form. But there was one difficulty about it which, though I tried till I almost made myself ill, I could not get over, and at last I had to give it up and profess myself unable to master it. I took it up again a few days ago, and after a two or three days' fight, the last of which I sat up till 3, I at last mastered it." (Stokes 1907b, p.62)

Stokes returned to the topic of asymptotic expansions again, very near the end of his life in a 1902 *Acta Mathematica* paper titled *On the Discontinuity of Arbitrary Constants that appear as Multipliers of Semi-convergent Series* (Stokes 1902). This was an invited paper for a collection put together to commemorate Abel in which Stokes chose to summarize three of his papers (1857, 1868, 1889) on asymptotic expansions published in the *Transactions of the Cambridge Philosophical Society.* He did this partly because he felt that, even though the papers had been available for a lengthy period, publication in *Transactions of the Cambridge Philosophical Society* made them not so widely know than if they had been published in a more widely read journal. This is an interesting comment which may bear upon why Stokes' work on asymptotic expansions was not influential and perhaps even indicates that Stokes was aware of this.

Stokes claimed that the three papers he summarized in 1902 were for the most part concerned with the complete integral of the differential equations of the second order which are satisfied by Bessel's functions.

Even given that we have seen that Stokes returned to and published on the topic of asymptotic series in 1857, I have not found any use of his method in the years immediately following the original publication of the method in 1856. A similar situation appears following the publication in 1864 on the now-called Stokes phenomenon. In the 1902 paper, Stokes described the discontinuous expressions for his asymptotic expressions with the variable expressed in polar format. Stokes said:

> “The way in which the paradox of giving a discontinuous expression for a continuous function is explained is this. A semi-convergent series (considered numerically, and apart from its analytical form) defines a function only subject to a certain amount of vagueness, which is so much the smaller as the modulus of the variable according to inverse powers of which it proceeds is larger. I have shown that, in general (i. e. for general values of θ), the vagueness of the superior function ultimately, as r is increased, disappears in comparison with the whole value of the inferior term. But for the critical values of θ for which the index of the exponential is real the vagueness of the superior function becomes sufficient to swallow up the inferior function. As θ passes through the critical value, the inferior term enters as it were into a mist, is hidden for a little from view, and comes out with its coefficient changed. The range during which the inferior term remains in a mist decreases indefinitely as the modulus r increases indefinitely.” (Stokes 1902, p.396)

As we shall see in the next two chapters, further development of asymptotic expansions came nearly thirty years later in 1886, in the work of Poincaré and Stieltjes. The work of these two mathematicians appears to be have been independent of Stokes’ work. As I will show, there were similarities between what Stokes did and what was to come — for example computations were needed in a physical situation (in this case celestial mechanics) — and there were differences (the level of rigour).

Chapter 4

Celestial mechanics, Mathematics and Poincaré

At the end of the nineteenth century, Henri Poincaré (1854-1912) was one of the world's leading researchers in both pure and applied mathematics as well as in physics. Poincaré's applied work encompassed many different fields including celestial mechanics, fluid mechanics, optics, electricity, quantum theory, relativity, and thermodynamics. He also wrote books on mathematics and physics for the general public as well as essays about the philosophy and methods of science.

According to Jeremy Gray (Gray 2012), Poincaré's intellectual output began in 1881 with contributions to the mathematics of complex function theory, complex differential equations and non-Euclidean geometry, which resulted in the theory of automorphic forms. During the late 1880s, the period of interest here, Poincaré made major contributions to the theory of real differential equations and provided, quoting Gray, "a radically new way" to handle celestial mechanics. His involvement in physics deepened as time progressed, though he continued to work in mathematics and, according to Gray, Poincaré produced his most lasting achievement in the creation of algebraic topology (Gray 2012, p.3).

It is perhaps possible to see Poincaré as primarily a mathematician who turned his attention to physics whereas, in contrast, it would not be possible to consider Stokes this way. Stokes was primarily interested in solving physical problems and he was adept at using and inventing new mathematical techniques for that purpose. Gray has provided a picture of Poincaré which captures Poincaré's outlook on science and mathematics and this outlook has understanding at its core. According to Gray, Poincaré thought that science was understanding based on axioms, principles and experimental data. Gray said:

> “We have no certainly beyond what shared use and discourse can guarantee, no unmediated access to reality. We do what we can according to our best understanding of the rules of the game (axioms, principles, the best experimental data). Mathematics and physics together offer us a rule-governed way of living in the world, although we may, from time to time, have to change the rules, and our ability to frame these rules is, in some ways, built into how our minds work.” (Gray 2012, p.2)

For basic biographical information and an in-depth analysis and summary of Poincaré’s scientific work see Jeremy Gray’s *Henri Poincaré: A Scientific Biography* (Gray 2012).

In this chapter and the next, I focus on Poincaré’s work in celestial mechanics and, in particular, on the use of divergent series in the computation of ephemerides. In this chapter, I use one of Poincaré’s philosophical works to provide context for the way in which Poincaré was thinking about the mathematics and physics he developed. I also discuss how this contrasted with what he saw of the British methodology of which Stokes was a canonical example.

4.1 Celestial Mechanics

The culmination of Poincaré’s work on celestial mechanics appeared in a three volume text titled *Les méthodes nouvelles de la mécanique céleste* which was published in 1892 (Volume I), 1893 (Volume II) and 1899 (Volume III). This work was translated into English, edited and published with an introduction by Daniel Goroff in 1993. I will cite from this English language translation (*New Methods in Celestial Mechanics*) (Poincaré & Goroff (ed.) 1993).

In the preface of this work on celestial mechanics, Poincaré clearly stated the purpose of his investigations. I summarize from this preface to indicate that purpose while highlighting the topics that are important to this thesis.

The major goal of celestial mechanics is to compute ephemerides of astronomical bodies using the laws of mechanics. During the 1880s and 1890s, that meant using Newton’s laws of gravitation to predict the orbits of various celestial objects. A difficult and important problem in celestial mechanics is the three body problem. This consisted of taking the initial positions and momenta of three point masses and solving for their future positions according to Newton’s law of universal gravitation. Poincaré stated the importance of the three-body problem in the first sentence of the preface to *New Methods in Celestial Mechanics*. Poincaré stated that the three-body problem was:

> "of such importance in astronomy, and is at the same time so difficult, that all efforts of geometers have long been directed toward it" (Poincaré & Goroff (ed.) 1993, p.xxi)

The process of exact integration to solve the differential equations of the three body problem was "manifestly impossible" (Poincaré & Goroff (ed.) 1993, p.xxi) and for that reason, approximation methods had to be used. I will show these methods use asymptotic expansions of divergent series.

The final goal of celestial mechanics, according to Poincaré, was to determine whether or not Newton's laws alone explain observed astronomical phenomena. The method of determining this was to predict, from Newtonian theory, using approximation methods if required, the position and velocity of astronomical bodies and then to use precise observations to validate those predictions. I emphasize that, just as in the case of Stokes and the pendulum, the closer the agreement between precise, observational measurements and good numerical approximations made from theory, the more confidence there was that the underlying theory was correct. Poincaré said it thus:

> "The only means of deciding [if Newton's laws alone explain all astronomical phenomena] is to make the most precise observations, and then compare them to calculated results. This calculation can only be approximate, and it would be pointless to calculate to more decimals than observation can give us. It is therefore useless to ask more precision from calculation than from observation, but neither should we ask for less. Furthermore, the approximation with which we can content ourselves today will be insufficient in several centuries. And, in fact, even admitting the improbability of perfecting measurement instruments, the very accumulation of observations over several centuries will permit us to know the coefficients of the various inequalities with greater precision." (Poincaré & Goroff (ed.) 1993, p.xxi)

Poincaré explicitly pointed out that the methods of Lagrange, Laplace and Urbain Jean Joseph Le Verrier (1811-1877) had been, until recently, sufficient for practical usage, and that those methods produced infinite series solutions where the argument was the masses of the heavenly bodies. These series solutions consisted of sine and cosine terms as well as a secular term in the time variable which occurred outside of the periodic terms. In astronomy, the secular term was used to identify the term of the solution which governed the long term behaviour of the system. For example, a solution with the form $f(t)(\cos(nt) + \sin(mt))$, where $f(t)$ is not periodic, has secular term $f(t)$. As a result, the convergence of solutions with secular terms was doubtful when the value of t was large. This had implications for the long term stability of the solar system.

Further, the presence of the secular term was, Poincaré claimed, an artifact of the solution method. He used the example of the Taylor series expansion of $\sin(\alpha mt)$ into $\alpha mt - \frac{(\alpha mt)^3}{6} + ...$ to demonstrate how a periodic solution could appear to but not actually have a secular term. If the solution to a differential equation were written as $\sin(\alpha mt)$ it would be clear that the solution was fully periodic. If however, the solution appeared or was written as an infinite series, $\alpha mt - \frac{(\alpha mt)^3}{6} + ...$ as in this example, it was more difficult to see that this a fully periodic solution. Further, it is possible to remove a common factor, t in this example, and write $t(\alpha m - \frac{(\alpha m)^3 t^2}{6} + ...)$ as the representation of the solution. This does not mean that there is a secular term, t — it is an artifact of how the solution was developed.

Charles-Eugène Delaunay (1816-1872), Hill, Johan August Hugo Gyldén (1841-1896) and Anders Lindstedt (1854-1939) had made improvements on the methods of Lagrange, Laplace and Le Verrier which succeeded in eliminating the secular terms. However, these improved methods did not produce infinite series that were convergent, Poincaré said, as understood by a mathematician. This appears to mean that, while a mathematician would define a series to be convergent if the partial sums tended to a limit, the astronomers were using series that were useful because they gave satisfactory approximations by truncation after the first term. The approximations were deemed satisfactory because they accorded with observation and the convergence of the series was not considered.

Even following the publication of Poincaré's work showing that series solutions to the equations of celestial mechanics were divergent, there was credible dissent about this. For example, Hill, in 1896, claimed that the reasons that Poincaré used to show that the series were divergent were not convincing, Hill said:

> "Recently M. Poincaré has much insisted that, under the latter condition, these series, in the rigorous mathematical sense, are divergent ... However, the reasons brought forward to sustain this opinion are scarcely convincing, and I think there has been some scepticism among astronomers in reference to the matter. Without attempting to find any flaw in M. Poincaré's logic, I simply wish to point out a class of cases where the convergency of the series can be shown." (Hill 1896a, p.93)

The confusion and lack of clarity regarding the convergence or divergence of the infinite series used in celestial mechanics during the 1890s is also evident in the 1889 paper of von

Bohlin (von Bohlin 1889) titled *Zur Frage der Convergenz der Reihenentwickelungen in der Störungstheorie* the subject of which was libration. Von Bohlin said:

> "Dass trotzdem die ersten Annäherungen stark convergiren, ist ohne weiteres klar; es muss aber dahingestellt bleiben, wie der erwähnte Umstand auf die späteren Annäherungen einwirkt, wenn Produkte von grossen ganzen Zahlen sich anhäufen. Gehören bei diesen Verhältnissen die Reihen, wenn sie divergiren, vielleicht zu den halbconvergenten? Und dies vorausgesetzt, welche Genauigkeit werden sie in verschiedenen Fällen (für verschiedene Integrationsconstanten) geben? Ist schliesslich die Tendenz zur Divergenz eine Anzeige, dass in allen Fällen Libration stattfindet? Dies alles sind Fragen, welche wir offen lassen müssen, an die sich aber sicher nicht nur ein theoretisches Interesse anknüpft."[1]
> (von Bohlin 1889, p.7)

Libration is an oscillation in the apparent aspect of a secondary body as seen from the primary object and von Bohlin was analyzing libration as a possible result of incommensurate orbits of the observing and observed celestial bodies.

The failure of the infinite series of celestial mechanics to converge meant that it was not possible to get arbitrarily close approximations for computed orbits from the infinite series. It also meant that fundamental questions about our solar system were not answerable using Newtonian theory and this included the question about the long term stability of the universe.

Poincaré, in the late 1880s, like Stokes forty years earlier, was investigating a physical problem modelled by differential equations which had no closed form solutions. Poincaré was concerned with the solutions to the differential equations of celestial mechanics whereas Stokes was concerned both with the differential equations of hydrodynamics as applied to the pendulum and as well as the differential equations of the wave theory of light.

Before looking in depth, in Chapter 5, at how Poincaré approached and used divergent series to get predictive numbers from the theory of celestial mechanics, I use *Science and Hypothesis* to show how Poincaré was thinking about his work and how he saw it as different from what the British did.

[1] That nevertheless the first approximations converge strongly is immediately clear; but it must be left undecided how the circumstance mentioned affects later approximations when products of large whole numbers accumulate. Under these conditions, do the series, if they diverge, perhaps belong to the half-convergent series? And given this, what precision will they give in different cases (for different constants of integration)? Finally, is the tendency to diverge an indication that libration is occurring in all cases? All of these are questions which we must leave open, but which are certainly not only of theoretical interest.

4.2 Science and Hypothesis

Poincaré's essay *Science and Hypothesis* was published in 1902. This book, written for a non-specialist audience, sheds light on how Poincaré thought about theory confirmation. In the preface of the work, Poincaré discussed the role of hypothesis in science. A hypothesis, according to Poincaré, is a proposed explanation made on the basis of limited evidence. It can be, among other things, the starting point for further investigation. Poincaré said:

> "We shall also see that there are several kinds of hypotheses; that some are verifiable, and when once confirmed by experiment become truths of great fertility; that others may be useful to us in fixing our ideas; and finally, that others are hypotheses only in appearance, and reduce to definitions or conventions in disguise. The latter are to be met with especially in mathematics and in the sciences to which it is applied." (Poincaré, Larmor & Greenstreet 1952, p.xxii)

The first English translation of *Science and Hypothesis* by William John Greenstreet (1861-1930) was published in 1902 with an introduction by Joseph Larmor (1857-1942) and an author's preface. A second English translation was published by Harvard University in 1905, translated by Bruce Halstead, and included an introduction by Josiah Royce (1855-1916). It was published with a new author's preface.

Larmor, Lucasian Professor of Mathematics at the University of Cambridge from 1903-1932, made a number of perceptive points in his introduction to Poincaré's essay about the role of experiment in theory and about the differences between the philosophy and methodology underlying French and British mathematical physics. His opinion was that in England, at that time, there was a growing trend of considering theoretical physical constructions as being artificially created or developed. Larmor said:

> "There has been of late a growing trend of opinion, prompted in part by general philosophical views, in the direction that the theoretical constructions of physical science are largely fictitious, that instead of presenting a valid image of the relations of things on which further progress can be based, they are still little better than a mirage." (Poincaré *et al.* 1952, p.xii)

Larmor felt that works like *Science and Hypothesis* were important to combat this type of thinking — that the results of science should be clearly explained to the public to ensure that science was seen to be explaining reality. Larmor said:

> "But much advantage will accrue if men of science become their own epistemologists, and show to the world by critical exposition in non-technical terms of the

> results and methods of their constructive works, that more than mere instinct is involved in it." (Poincaré *et al.* 1952, p.xii)

Larmor claimed there was a difference in culture between the English and the French in this regard and we shall see this difference in the approaches of Stokes and Poincaré to divergent series. There were the "close-knit theories of the classical French mathematical physicist" (Poincaré *et al.* 1952, p.xiv) which contrasted with the "somewhat loosely connected *corpus* of ideas" (Poincaré *et al.* 1952, p.xiv) of the British.

Two examples of this difference were given by Larmor. The first compared the French conception of the ether with that of Maxwell and the second noted the difference between Laplace and Thomas Young (1773-1829) with regard to the theory of capillarity. Laplace started with fixed conceptions regarding atomic forces and, from that, logically developed his argument, whereas Young used "tentative, mobile intuitions" (Poincaré *et al.* 1952, p.xv). The end result was the Young-Laplace non-linear partial differential equation that described the pressure difference across the interface of two static fluids.

Larmor felt that Young's method allowed him to grasp the fruitful, though partial, analogy between capillary pressure and other more familiar physics in a way that the elaborate analytical theories of Laplace did not. Further Larmor felt that the approach of Young and Laplace were, at least partially, mutually incomprehensible. Larmor said:

> "The *aperçus* of Young were apparently devoid of all cogency to Laplace; while Young expressed, doubtless in too extreme a way, his sense of the inanity of the array of mathematical logic of his rival." (Poincaré *et al.* 1952, p.xv)

This, of course, was an opinion of Larmor and not everyone would necessarily agree with this analysis. Fox, in a piece titled *The Rise and Fall of Laplacian Physics* (Fox 1974), described the Laplacian program that dominated French physics in the period roughly between 1799 and 1815 as one in which all physical phenomena were seen as a result of repulsive and attractive interactions between particles. Further, Fox claimed, optical refraction and capillary action were among the first two subjects of Laplace's entry into molecular physics (Fox 1974, p.100). Fox said:

> "In this earliest work on his program Laplace gave lengthy mathematical treatments of optical refraction and capillary action, basing both treatments on the supposed existence of short-range attractive forces of the type that had been first postulated by Newton and discussed so often through the eighteenth century." (Fox 1974, p.100)

According to Fox, the Laplacian program, in France, crumbled quickly beginning in roughly 1815. Young, however, was working in England and that allowed for criticism of the prevailing theories of heat and light to start earlier, near the beginning of the nineteenth century. Fox said:

> "Significantly, too, it was in England that the freer, if less stimulating, intellectual climate allowed serious criticism of the imponderables of heat and light to get under way by the first years of the nineteenth century (in the writings of Rumford, Davy, and Young)." (Fox 1974, p.134)

A new physical theory can start as an analogy with a mechanical dynamical system and then mature into a problem of mathematical physics where it no longer matters if the mechanical analogy survived. This method was a predilection of the British according to Poincaré.

Poincaré's work on divergent series emerged in France, roughly forty years after the work of Stokes. Poincaré's work, as seen in his writing on celestial mechanics and pure mathematics, consisted more of definitions and logical conclusions than the work of Stokes which was more pragmatic — it gave results that verified theory.

Larmor's commentary on the difference in the thought processes between Anglo-Saxons and Latin peoples was echoed in Poincaré's preface to the second English translation of *Science and Hypothesis*. Most of this preface is an exposition on the topic of national, or perhaps even racial difference, with commentary on the advantages and disadvantages of the two different approaches. Poincaré argued that the Latin (i.e. French) approach was more theoretical, logical, and careful whereas the Anglo-Saxon (i.e. British and American) approach was more pragmatic, intuitive, and less careful. A series of quotes from this preface makes clear how Poincaré viewed the differences in style. I claim that this description is an accurate depiction of the differing approach of Stokes and Poincaré to handling the divergent series that emerged in various solutions to physical problems.

I am interested here in drawing attention to the differing scientific practices of France and England and I am claiming that we can see that difference in the approaches of Poincaré and Stokes. The fact that these quotes draw attention to the issue that race is being grafted onto scientific practice, while not unimportant, is not the point I am making. The British, claimed Poincaré, tended to proceed from the particular to the general whereas the French proceeded from the general to the particular. Poincaré said:

> "The English, even in mathematics, are to proceed always from the particular to the general, so that they would never have an idea of entering mathematics, as

> do many Germans, by the gate of the theory of aggregates. They are always to hold, so to speak, one foot in the world of the senses, and never burn the bridges keeping them in communication with reality. They thus are to be incapable of comprehending or at least of appreciating certain theories more interesting than utilitarian, such as the non-Euclidean geometries." (Poincaré 1905, p.4)

The theory of aggregates, or set theory (Mengenlehre in German) as it is now called, became foundational in mathematics during the nineteenth century primarily as a result of the work of Georg Ferdinand Ludwig Philipp Cantor (1845-1918) and, to a lesser extent, Julius Wilhelm Richard Dedekind (1831-1916). Cantor's theory was replaced in the early twentieth century by axiomatic Zermelo-Fraenkel set theory. Starting mathematical studies with the study of set theory is to start with an abstract foundation that is not referenced in any way to the physical world. Poincaré further claimed that this contrast between the French and the British in the importance placed on the primacy of the general versus the particular was even more acute when physical, as opposed to solely mathematical, problems were under consideration. Poincaré further said:

> "In the study of nature, the contrast between the Anglo-Saxon spirit and the Latin spirit is still greater. The Latins seek in general to put their thought in mathematical form; the English prefer to express it by a material representation. Both doubtless rely only on experience for knowing the world; when they happen to go beyond this, they consider their foreknowledge as only provisional, and they hasten to ask its definitive confirmation from nature herself." (Poincaré 1905, p.5)

Here, Poincaré claimed that, for the British, the model was more important than the equations and that the reverse was true for the French. Looking back to Stokes' explanation of the index of friction, we see this — he provided a drawing in the initial explanation to show how he was thinking about the physical situation rather than providing the equations directly. This type of physical explanation is absent from the work of Poincaré whose discussion, as we shall see, started with definitions from which rigorous mathematics was developed in a logical manner. Poincaré explained that:

> "For a Latin, truth can be expressed only by equations; it must obey laws simple, logical, symmetric and fitted to satisfy minds in love with mathematical elegance. The Anglo-Saxon to depict a phenomenon will first be engrossed in making a model, and he will make it with common materials, such as our crude,

unaided senses show us them. He also makes a hypothesis, he assumes implicitly that nature, in her finest elements, is the same as in the complicated aggregates which alone are within the reach of our senses. He concludes from the body to the atom." (Poincaré 1905, p.5)

After Poincaré clearly explained what he felt the differences in approach were, he went on to analyze the advantages and disadvantages of the British method over that of the French. He felt that the British approach, in some sense, was indicative of muddled thinking that rigorous mathematics could have fixed. On the other hand, by placing less emphasis on mathematical detail, the British were able to make large leaps that led to important results that the French did not make as they focussed on the mathematical minutiae of a problem. Poincaré said:

> "The English procedure often seem to us crude, the analogies they think they discover to us seem at times superficial; they are not sufficiently interlocked, not precise enough; they sometimes permit incoherences, contradictions in terms, which shock a geometric spirit and which the employment of the mathematical method would immediately have put in evidence. But most often it is, on the other hand, very fortunate that they have not perceived these contradictions; else would they have rejected their model and could not have deduced from it the brilliant results they have often made to come out of it. And then these very contradictions, when they end by perceiving them, have the advantage of showing them the hypothetical character of their conceptions, whereas the mathematical method, by its apparent rigor and inflexible course, often inspires in us a confidence nothing warrants, and prevents our looking about us." (Poincaré 1905, p.6)

In the introduction to the 1905 translation of *Science and Hypothesis*, Royce stated his summary of the Poincaré viewpoint of the French method. It was a method, Royce claimed, that had as a basis a set of mathematical statements which, while not arbitrary, were also not given as a result of experience with the physical world. Royce said:

> "...we consequently have in M. Poincaré's account a set of conventions, neither wholly subjective and arbitrary, nor yet imposed upon us unambiguously by the external compulsion of experience." (Poincaré 1905, p.xxiii)

The content of *Science of Hypothesis*, the two introductions by Royce and Larmor, and most compellingly, the preface that Poincaré wrote for the 1905 translation give insight into how mathematical physics was practiced and conceived of differently in Britain and France. It is not therefore surprising to see, in the next chapter, exactly these types of differences in the approach to divergent series between the work of Stokes and that of Poincaré.

Again, I reiterate that the discussion above is presented to show that there was a real difference in practice between the French and the English. I am not commenting on or concluding anything from the way in which these differences or the reasons for them were presented by Poincaré, Larmor, or Royce who sometimes framed them, as was the convention of the day, in terms of hereditary factors, explicitly or otherwise.

Chapter 5

Poincaré and the Use of Divergent Series

5.1 The Three Body Problem and the Equations of Dynamics

In 1889, in celebration of the sixtieth birthday of King Oscar II of Sweden, there was a prize competition. Poincaré submitted an entry to this competition that concerned the stability of the solar system. His entry, specifically, was an analysis of the three body problem.

The three body problem, at that time, consisted of determining the future paths of three point masses, given their initial positions and momenta, as they move under mutual gravitational attraction. It is a special case of the n body problem. The three body problem can be simplified by making one of the three masses negligible such that it does not exert any gravitational force on the other two masses. This is called the restricted three body problem. The earth, moon, and sun system, for example, can be modelled well as a restricted three body problem when the mass of the moon is neglected.

Poincaré chose to study the three body problem, in part, because the n body problem was too complex and the two body problem had already been solved with a closed form solution by Isaac Newton in 1687. Further, the three body problem applies to the familiar sun, earth and moon system.

The first manuscript that Poincaré prepared for the prize competition was not what was published by the prize committee when Poincaré was awarded the prize. The history of Poincaré's competition entry is interesting and June Barrow-Green's (Barrow-Green 1996) book, *Poincaré and the Three Body Problem*, is the authority on this. In this thesis, I will use the final version of *The Three-Body Problem and the Equations of Dynamics*, published in *Acta Mathematica* in 1890, as I focus on how Poincaré handled the divergent series of celestial mechanics. Recently, in 2017, the final version of Poincaré's entry to the competition was translated into English by Bruce Popp (Poincaré & Popp 2017). I have used that translation here.

Prior to the work of Poincaré, it was generally assumed that the motion of the planets could be accurately predicted from Newton's deterministic laws of motion under the force of gravity as explained by Newton's theory of gravitation. The discovery of Neptune, in 1846, extremely close to its theoretically predicted location, was a major triumph of the use of Newton's theory of gravitation as well as for the computational methods of celestial mechanics at the time.

> "It was a moment of a kind that had not been expected, whereby the obscure mathematics of perturbation-theory pinpointed the position of a new sphere in the sky." (Kollerstrom 2006)

The computations involved in predicting future orbits of any planetary system of more than two objects require series expansions. Over short time domains, these computations produced observationally verifiable results. Prior to Poincaré, the convergence of these series over longer time periods was either assumed or unknown. Further, there was no proof that Newton's laws of motion and Newton's law of universal gravitation were sufficient in and of themselves to explain planetary motion. This meant, among other things, that it was unknown whether or not the orbits of the planets in our solar system were stable or not. This was a question of religious significance and major cultural resonance. Poincaré investigated this questions as the subject of his entry to the prize competition.

Poincaré showed that the series being used in celestial mechanics during the latter part of the nineteenth century were divergent and therefore existing methods could not be used to determine solar system stability. Further Poincaré showed that convergence of the series solutions to the differential equations of celestial mechanics could not, in general, be established over long time frames. Thus, it was unknown whether or not the solar system was stable. Poincaré worked on the stability question in a variety of ways that will not be discussed in this thesis.

There are different ways to define orbital stability, and dynamical systems stability in general, and two different conceptions appear in the work of Poincaré. One of those definitions required that the computed approximations of orbits stay bounded in a neighbourhood of an elliptical orbit. This was the definition used by Lagrange and Laplace. In contrast, Poisson defined stability to mean that a predicted orbit could stray far from the periodic orbit but at some point the celestial body must return to the initial starting position. This is called Poisson stability or, as Poincaré referred to it, stability in the sense of Poisson. For a fuller discussion of the history of the definition of stability see (Roque 2011).

I will focus on how Poincaré handled divergent series in his work on celestial mechanics. To tackle the three body problem, Poincaré started by studying the general equations of dynamics in their Hamiltonian form. This meant that results that he found in the context of the three body problem extended well beyond questions in celestial mechanics and provided a basis for later work in dynamical systems theory.

As Poincaré acknowledged in the author's preface of (Poincaré & Popp 2017), he was a long way from fully resolving the stability problem at the conclusion of his analysis of the three body problem. The success he did have came from his demonstration of the existence of some specific types of solutions which he called periodic solutions, asymptotic solutions, and doubly asymptotic solutions. Further, his close analysis of the restricted three body problem produced rigorous results and showed that the three bodies return arbitrarily close to their starting positions infinitely many times meaning that the solutions were stable in the Poisson sense. It also meant the three bodies should be repeatedly observable at given locations.

This result was generalized shortly afterwards and became the Poincaré recurrence theorem which stated that certain dynamical systems will return to positions arbitrarily close to their initial state in finite time. It was discussed in the context of celestial mechanics, as we see here, in 1890 by Poincaré, and was proved by Constantin Carathéodory (1873-1950) in 1919.

A significant portion of Poincaré's work on the three body problem, and the portion on which I will focus, involves asymptotic analysis of divergent series. Poincaré showed that most of the series used in celestial mechanics were divergent but that useful values were still obtainable from these series. Poincaré, like Stokes did, immediately drew an analogy to the Stirling series and pointed out that prior, perhaps unwitting, use of divergent series by Lindstedt and Gyldén had produced useful results. Poincaré said it thus:

> "I also show that most of the series used in celestial mechanics and in particular those of Mr. Lindstedt, which are the simplest, are not convergent. I am sorry in that way to have thrown some discredit on the work of Mr. Lindstedt or on the more detailed work of Mr. Gyldén; nothing could be farther from my thoughts. The methods that they are proposing retain all their practical value. In fact the value that can be drawn from a numeric calculation using divergent series is known and the famous Stirling series is a striking example. Because of an

analogous circumstance, the tried-and-true developments in celestial mechanics have already rendered such great service and are called on to render even greater service." (Poincaré & Popp 2017, p.xx)

This was an acknowledgement by Poincaré that divergent series had been used in celestial mechanics prior to his study and that the results obtained had been useful where useful meant that the results were predictions that were verified by astronomical observation.

Part 1 of the memoir *The Three Body Problem and the Equation of Dynamics*, consisting of 125 pages, was a review which covers the general properties of differential equations, the theory of integral invariants and the theory of periodic solutions. In each section of Part 1, solutions to differential equations were developed under a variety of constraints and then, in each case, the convergence of the resulting series was investigated. Until the section on asymptotic solutions, which appeared near the end of the theory of periodic solutions, all of the series solutions of the differential equations were convergent.

Poincaré, as the memoir progressed, identified an infinite series that formally satisfied a given differential equation but was not convergent. The radius of convergence of this series approached zero under certain circumstances and further, there were infinitely many quantities for which the radius of convergence of the series solution expansion was as small as desired. Then Poincaré asked:

"But even though they are divergent cannot we get something from them?" (Poincaré & Popp 2017, p.133)

At this point, Poincaré did something exactly like Stokes did — he asked the reader to consider a simpler series, for which he then discussed the convergence. The series Poincaré chose for his example was:

$$F(w, \mu) = \sum_n \frac{w^n}{1 + n\mu}$$

which is a two variable function defined as an infinite power series in w. This function and its infinite series representation was solely a mathematical example; it did not come from celestial mechanics.

This series converges uniformly when μ is positive and w remains smaller in absolute value than some positive number w_0 smaller than 1. This was roughly forty years later than when Stokes did his work on divergent series and uniform convergence was now well established so, unlike Stokes, Poincaré differentiated between uniform and pointwise convergence.

Poincaré used the uniform convergence of this series to differentiate (Poincaré did not use partial derivative notation and I have retained his notation) this series with respect to μ term by term and claimed that

$$\frac{1}{p!}\frac{\mathrm{d}^p F(w,\mu)}{\mathrm{d}\mu^p} = \pm \sum \frac{n^{p-1} w^n}{(1+n\mu)^p}$$

was similarly convergent. There is a small error in the exponent of n in the published work and term by term differentiation of $F(w,\mu)$ shows that what is actually similarly convergent is the function

$$\frac{1}{p!}\frac{\mathrm{d}^p F(w,\mu)}{\mathrm{d}\mu^p} = \sum_n (-1)^p \frac{n^p w^n}{(1+n\mu)^{p+1}}$$

Next, Poincaré expanded $F(w,\mu)$ via a Taylor series expansion in powers of μ by using the derivatives above as the coefficients in the Taylor series and obtained the resulting series

$$\sum w^n (-n)^p \mu^p$$

Poincaré's small error in the computation of the derivatives above cancelled out and does not affect the result above. The summation index above, not explicitly stated by Poincaré, is p. Also, this expression is to be taken to mean a double summation which I write as

$$F(w,\mu) = \sum_p \left(\sum_n w^n (-n)^p \right) \mu^p$$

Recall that the only constraint on μ in the original convergent infinite seres was that μ remain positive. Therefore this is an alternating series whose terms do not tend to zero. Thus it is clearly not convergent.

This was a mathematical example — F was not a function resulting from modelling a physical situation and neither was it referenced to any of the earlier analysis of the series that were identified as solutions of the differential equations of celestial mechanics. F is simply a function defined by a convergent power series. That this power series was taken and turned into another power series which was divergent provided a simple, uncluttered example of the convergence issues that appeared when using the series that arose in celestial mechanics.

Next Poincaré truncated the power series such that the terms when exponent of μ is greater than p are 'neglected' (as Poincaré said). This gave a finite function consisting of the first p terms of F which Poincaré called $\Phi_p(w, \mu)$. The difference between this finite function and the original function, scaled by $\frac{1}{\mu^p}$ is:

$$\frac{F(w, \mu) - \Phi_p(w, \mu)}{\mu^p}$$

Taking the limit of the above expression as μ approaches 0 from the right, Poincaré found that $\Phi_p(w, \mu)$ asymptotically represented $F(w, \mu)$ for small values of μ in the same way that the Stirling series asymptotically represents the Euler gamma function for large values of x.

Poincaré, again, clearly drew this analogy to the Stirling series and stated that the divergent series that arose in his analysis of the dynamical systems equations were exactly of this sort. For example, series developed earlier in the memoir:

$$\sum \frac{N}{\Pi} w_1^{\beta_1} w_2^{\beta_2} \ldots w_k^{\beta_k} e^{\gamma t \sqrt{-1}} = F\left(\sqrt{\mu}, w_1, w_2, \ldots w_k, t\right)$$

and

$$\sum w_1^{\beta_1} w_2^{\beta_2} \ldots w_k^{\beta_k} e^{\gamma t \sqrt{-1}} \frac{\mathrm{d}^p \left(\frac{N}{\Pi}\right)}{(\mathrm{d}\sqrt{\mu})^p} = \frac{\mathrm{d}^p F}{(\ \mathrm{d}\sqrt{\mu})^p}$$

are uniformly convergent if w remains smaller in absolute value than some bound and $\sqrt{\mu}$ remains real. However, if $\frac{N}{\Pi}$ is expanded in powers of $\sqrt{\mu}$, then the resulting series are divergent.

Again, Poincaré truncated to a polynomial of degree p, here in powers of $\sqrt{\mu}$, to get a function:

$$\Phi_p\left(\sqrt{\mu}, w_1, w_2, \ldots w_k, t\right)$$

expandable in powers of w and $e^{\pm t \sqrt{-1}}$. By the same logic, $F\left(\sqrt{\mu}, w_1, w_2, \ldots w_k, t\right)$ is asymptotically represented by Φ_p, however large p is when μ approaches 0 from the right.

Poincaré then stated yet again that series he previously obtained in the memoir are asymptotic solutions in the same manner as the Stirling series asymptotically represent the Euler gamma function. At this point, Poincaré expended additional effort to make this idea understood. He started with a simplified differential equation example with just two degrees of freedom and kept just the quantity w which gave equations of the form:

$$\frac{\mathrm{d}x_i}{\mathrm{d}t} + \alpha w \frac{\mathrm{d}x_i}{\mathrm{d}w} = \frac{\mathrm{d}F}{\mathrm{d}y_i}, \quad \frac{\mathrm{d}y_i}{\mathrm{d}t} + \alpha w \frac{\mathrm{d}y_i}{\mathrm{d}w} = -\frac{\mathrm{d}F}{\mathrm{d}x_i} \quad (i = 1, 2)$$

Because α is expandable in odd powers of $\sqrt{\mu}$, α^2 is expandable in powers of μ. Conversely μ is expandable in powers of α^2. Thus F can be expanded in powers of α^2. Letting $\alpha = 0$, reduces F to F_0 and the periodic solutions that result

$$x_i = \varphi_i(t), \quad y_i = \psi_i(t)$$

serve as a starting point. This was what Stokes did as well — simplify the differential equation and find the periodic solution to the simplified differential equation. Stokes then assumed that the solution to the original differential equations was the periodic solution multiplied by a descending series. Here, Poincaré similarly decided that the complete solution was the periodic solutions plus something else. Poincaré added a perturbation to the periodic solutions which yielded

$$x_i = \varphi_i(t) + \xi_i, \quad y_i = \psi_i(t) + \eta_i.$$

These solutions were substituted back into the differential equations which gave

$$\frac{\mathrm{d}\xi_i}{\mathrm{d}t} + \alpha w \frac{\mathrm{d}\xi_i}{\mathrm{d}w} = \Xi_i, \quad \frac{\mathrm{d}\eta_i}{\mathrm{d}t} + \alpha w \frac{\mathrm{d}\eta_i}{\mathrm{d}w} = \mathrm{H}_i.$$

These new equations were analyzed over several pages where at each stage it was stated which equations this simplified example was being compared to from the earlier analysis of asymptotic expansions. As before, convergent series initially resulted which, when transformed in the manner explained above, become divergent series. Poincaré showed and emphasized that the solution functions he gave were asymptotic solutions, again in the manner of the Stirling series. Poincaré stated this for a third time on page 142. By this he meant that a series

$$\theta_i^0 + \alpha\theta_i^1 + \alpha\theta_i^2 + \cdots$$

represented the function θ_i *asymptotically*, emphasis in the original, when the expression

$$\frac{\theta_i - \theta_i^0 - \alpha\theta_i^1 - \alpha\theta_i^2 - \cdots \alpha^{p-1}\theta_i^{p-1}}{\alpha^{p-1}}$$

approached zero with α.

It was here, at the end of the section on the theory of periodic solutions, that Poincaré referred the reader to Section 1 of his paper published in the eighth volume of *Acta Mathematica* for more series analogous to the Stirling series. This is the article *Sur Les Intégrales irrégulières des équations linéaires* (Poincaré 1886) analyzed later in this chapter.

Repeated emphasis on the similarity of these series to the Stirling series is interesting. Poincaré emphasized that he knew he was starting with a convergent series, that he then did some manipulations that resulted in a divergent series, and that the reader should be comfortable with this because it is no different than using the Stirling series to calculate values of the gamma function. It was a non-mathematical argument that appealed to familiarity with something widely used.

Poincaré's memoir continued with Part II, titled *Equations of Dynamics and the N-body Problem*, which started with a study of the restricted three body problem. It was here that Poincaré supported his work with three physical examples. The first example was the restricted three body problem with two masses, one large, one very small and finite such that these bodies "describe around their mutual centre of gravity a circumference of uniform motion" (Poincaré & Popp 2017, p.150). The third body was of infinitesimal mass so that it did not perturb the motion of the first two bodies. Further the third body was in the plane containing the circumference of motion of the other two bodies. There are two cases like this in our solar system:

1. A small planet moving under the influence of the Sun and Jupiter when the eccentricity of Jupiter is neglected and the inclination of the orbits are neglected.
2. A similar example to Example 1 consisting of the Sun, Earth and Moon, where the eccentricity of the Earth's orbit is neglected and the inclination of the Moon's orbit to the ecliptic are neglected.

Analysis of these cases resulted in asymptotic solutions which Poincaré then represented geometrically with trajectory curves. At this point he took care to remind the reader of his definition of an asymptotic solution as well as the allowed rules of calculation with an asymptotic solution. Poincaré said:

"Remember that I agreed to state that the series

$$A_0 + A_1 x + \cdots A_p x^p + \cdots$$

asymptotically represents the function $F(x)$ for very small x, when

$$\lim_{x \to 0} \frac{F(x) - A_0 - A_1 x - \cdots A_p x^p}{x^p} = 0$$

> In Acta Mathematica, Volume 8, I studied the properties of divergent series which asymptotically represent certain functions and I recognized that the ordinary rules of calculation are applicable to these series. An equality, meaning an equality between a divergent series and a function that it represents asymptotically, can undergo all the ordinary calculation operations, except for differentiation." (Poincaré & Popp 2017, p.162)

The family of the trajectory curves that represent the asymptotic solutions form a surface which Poincaré called an asymptotic surface. Most of the remainder of *The Three Body Problem and the Equation of Dynamics* continues with an analysis of asymptotic surfaces. This part of Poincaré's analysis, based on a successive approximation strategy, is beyond the scope of this thesis but it is this analysis which was used to understand the stability of the solutions — one of the major objectives of the work.

There is interplay between convergence and stability and Poincaré defined several different types of stability. As we saw earlier, Poisson defined a stable planetary orbit to mean that the planet returned to its initial position infinitely many times. This definition does not preclude the orbit growing arbitrarily large (i.e $t \cos \omega t$). This is different than defining stability of an orbit of the planet as staying within a certain bounded region of space. Further, there was a distinction between secular stability and temporary stability. The $t \cos \omega t$ type terms do not have secular stability but if t is small and the solutions remain bounded for those values of t, then there is temporary stability.

The final topics of *The Three Body Problem and the Equation of Dynamics* include several impossibility proofs. First Poincaré proved that series produced by Lindstedt are not convergent — that it, it is not possible for them to converge. Second, he proved the nonexistence of one-to-one integrals as solutions to the equations of dynamics.

The final chapter of the memoir was an attempt to generalize the three body problem results to the n body problem which Poincaré concluded was not possible without additional effort. This conclusion surprised him because, when he started his analysis of the three body problem, he had expected to be able to generalize immediately to the n body problem. Poincaré said:

> "I believed when I started this work that once the solution of the problem was found for the specific case that I dealt with it would be immediately generalizable without having to overcome any new difficulties outside of those which are due to the larger number of variables and the impossibility of a geometric representation. I was mistaken." (Poincaré & Popp 2017, p.237)

After discussing some of the difficulties encountered while trying to generalize to the n body problem, Poincaré concluded that he did not have sufficient time for the generalization and moreover it was premature even to try this at this time. I now turn to the solely mathematical paper that Poincaré referred to in the prize competition entry.

5.2 Poincaré's 1886 *Acta Mathematica* Paper

Sur les intégrales irrégulières des équations linéaires (Poincaré 1886) was published in *Acta Mathematica* on December 1, 1886, three years prior to the memoir on the three body problem. It was referenced several times in the three body problem memoir and has not, to my knowledge, been translated into English. It is in this paper that Poincaré first explained his method of finding and handling divergent series. I will discuss it in some detail.

Sur Les Intégrales irrégulières des équations linéaires started by reminding the reader of the curious properties of the Stirling series, known to all mathematicians. The Stirling series, divergent for all values of the argument, consists of terms which initially decrease rapidly and then increase without bound. By truncating the series at the smallest term, the error in using the asymptotic expansion is minimized. Poincaré said:

> Tous les géomètres connaissent les curieuses propriétés de la série de Stirling. Cette série:
>
> $$\log\Gamma(x+1) = \frac{1}{2}\log(2\pi) + \left(x+\frac{1}{2}\right)\log x - x + \frac{B_1}{1\cdot 2}\frac{1}{x} - \frac{B_2}{3\cdot 4}\frac{1}{x^2} + \frac{B_3}{5\cdot 6}\frac{1}{x^3} - \dots$$
>
> est toujours divergente. Cependant on peut en faire légitimement usage pour les valeurs très grandes de x. En effet les termes après avoir décru avec une très grande rapidité, croissent ensuite au delà de toute limite. Mais si l'on s'arrête au plus petit terme, l'erreur commise sur la valeur de $\log\Gamma(x+\mathrm{I})$ est très petite. (Poincaré 1886, p.295)[1]

The B_i in the Stirling series are the Bernoulli numbers which were discovered in the early eighteenth century and named after Jacob Bernoulli (1655-1705).

[1]All geometers know the curious properties of the Stirling series. This series: $\log\Gamma(x+1) = \frac{1}{2}\log(2\pi) + \left(x+\frac{1}{2}\right)\log x - x + \frac{B_1}{1\cdot 2}\frac{1}{x} - \frac{B_2}{3\cdot 4}\frac{1}{x^2} + \frac{B_3}{5\cdot 6}\frac{1}{x^3} - \dots$ is always divergent. However, it can be legitimately used for very large values of x. Indeed the terms after decreasing with very great rapidity then increase beyond any limit. But if we stop at the smallest term, the error made on the value of $\log\Gamma(x+1)$ is very small.

Clearly the Stirling series provided a starting point for Poincaré in his discussion of divergent series. There was no mention in the 1886 paper about why it was of interest to analyze divergent series and, in particular, there was no mention of the equations that govern celestial mechanics. This is a mathematical paper only. This naturally raises the question of why Poincaré was working on this at this time. I conjecture that, even though there was no explicit statement of this, it was because of the research he was doing in celestial mechanics. This paper was likely written to encapsulate the new mathematics that Poincaré was going to make use of in his work on the three body problem and allowed him to express it in a more fulsome way.

As we saw earlier, in the discussion of the three body memoir, Poincaré took a given function of two variables, $F(w, \mu)$, which was convergent when expanded in powers of w and showed it to be divergent when expanded in powers of μ. This example mimicked the behaviour of the type of divergent series that appeared when analyzing the solutions to the differential equations of celestial mechanics.

In *Sur Les Intégrales irrégulières des équations linéaires* Poincaré took a different approach. He first claimed that there are obviously an infinite number of series whose terms, after decreasing very rapidly, increase beyond any limit. Poincaré created a family of these series in the manner described below.

First, he took a sequence of numbers, all less than 1, and labeled those A_1 ... A_n where the $\lim_{n\to\infty} A_n$ was not zero. He then considered the series:

$$\frac{A_1}{x} \cdot 1 + \frac{A_2}{x^2} \cdot 1 \cdot 2 + \cdots + \frac{A_n}{x^n} \cdot 1 \cdot 2 \cdot 3 \cdots n + \cdots$$

When x is large, the terms decrease quickly but as n grows, the terms of the series eventually become greater than 1 and the series consequently diverges.

Next, he analyzed the series for $n = x$ and considered the n^{th} term which is

$$\frac{A_n}{n^n} n!$$

and can be bounded above in the following manner:

$$\frac{A_n}{n^n} 1.2 \ldots n < 1 \left(1 - \frac{1}{n}\right) \left(1 - \frac{2}{n}\right) \ldots \left(1 - \frac{n-1}{n}\right) < ne^{-n}$$

For large n, and thus large x, the upper bound ne^{-n} is extremely small.

Next Poincaré considered a divergent series of the form

$$J(x) = A_0 + \frac{A_1}{x} + \frac{A_2}{x^2} + \cdots + \frac{A_n}{x^n} + \ldots$$

where the partial sum S_n is the sum of the first $n+1$ terms. Poincaré then defined S_n to be the asymptotic representation of function $J(x)$ if the expression

$$x^n \left(J - S_n\right)$$

tended to zero when x increased indefinitely. Poincaré said:

> "En effet si x est suffisamment grand, on aura
>
> $$x^n \left(J - S_n\right) < \varepsilon$$
>
> ε étant tres petit; l'erreur
>
> $$J - S_n = \frac{\varepsilon}{x^n}$$
>
> commise sur la fonction J en prenant les $n+1$ premiers termes de la série est alors extrâmement petite. De plus, elle est beaucoup plus petite que l'erreur commise en prenant seulement n termes et qui est égale à:
>
> $$J - S_{n-1} = \frac{A_n + \varepsilon}{x^n}$$
>
> ε étant très petit et A_n fini."[2] (Poincaré 1886, p.296)

Again, Poincaré compared this with the formula of Stirling and claimed that similarly, for large values of the argument x, the terms of series decrease rapidly and then increase without bound but that it is still legitimate to use the series. Poincaré said:

> "si x est trés grand, ses termes décroîtront d'abord rapidement pour croître ensuite au delà de toute limite et que, malgré sa divergence, il sera légitime de

[2]In effect, if x is sufficiently large, we will have $x^n \left(J - S_n\right) < \varepsilon$, ε being very small; the error$J - S_n = \dfrac{\varepsilon}{x^n}$ committed on the function J by taking the first $n+1$ terms of the series is then extremely small. Moreover, it is much smaller than the error made by taking only n terms and which is equal to:$J - S_{n-1} = \dfrac{A_n + \varepsilon}{x^n}$, ε being very small and A_n finite.

> s'en servir dans le calcul de J. Je dirai aussi quelquefois pour abréger que la série ... est une série asymptotique."[3] (Poincaré 1886, p.297)

Poincaré used the term asymptotic series here in a manner that suggests he is introducing the term to the reader for what he has just mathematically defined. Stokes used the term descending series. There have been other terms used — Stieltjes, as we shall see, used the term semi-convergent series, also in 1886 in his doctoral thesis, and the term convergently beginning series has been used in numerical analysis papers but it is the terminology that Poincaré coined that is now generally used.

For example, Arthur Erdélyi (1908-1977) used the terms asymptotic series and asymptotic expansions in the 1954 book *Asymptotic Expansions* (Erdélyi 1956). The stated purpose of this text was twofold: first, to introduce students to asymptotic evaluations of integrals containing a large parameter, and, second, to find solutions of ordinary linear differential equations by means of asymptotic expansions. The first goal of Erdélyi's text was Stokes' goal in 1848 and the second goal of Erdélyi's text was Poincaré's 1886 goal.

Erdélyi claimed that the theory of asymptotic series was initiated by Stieltjes and Poincaré in 1886. He is referring to this 1886 paper of Poincaré and the thesis of Stieltjes which is analyzed in the next chapter of this thesis. There is no mention of Stokes in Erdélyi's book outside of the mention of Stokes lines. It would not be unreasonable for an author to ask why the locations of the discontinuities in the parameters of asymptotic expansions bears the name of Stokes had he not developed some of the mathematics of asymptotic expansions.

Erdélyi distinguished between asymptotic series, which I have referred to as summability theory, and asymptotic expansions — the construction and investigation of a series which represents a given function asymptotically. These functions are, Erdélyi said:

> "often given by integral representations, or by power series, or else appear as solutions of differential equations; and in the latter case the 'variable' of the asymptotic expansions may occur either as the independent variable, or else as a parameter, in the differential equation". (Erdélyi 1956, p.4)

Poincaré continued *Sur Les Intégrales irrégulières des équations linéaires* with proofs of the following properties of asymptotic expansions:

[3] if x is very large, its terms will first decrease rapidly and then increase beyond any limit and that, despite its divergence, it will be legitimate to use in the calculation of J. I will also say sometimes for short that the series ... is an asymptotic series.

1. The product of the asymptotic expansions of two functions is the asymptotic expansion of the product of the two functions. As we shall see in Chapter 6, Stieltjes wrote a paper about the product of two asymptotic expansions in 1887.

2. As a consequence of property one, it is possible to compute any power of an asymptotic approximation and obtain an asymptotic approximation of the function raised to that power.

3. An asymptotic series can be substituted in the development of a holomorphic function as if it were convergent. I understand this to mean that if a holomorphic function has, as a part, a function for which an asymptotic expansion exists, this expansion can be substituted into the function.

4. Two asymptotic series can be divided provided division by zero is avoided.

5. Asymptotic series can be integrated term-by-term, with the result being the integral of the original function, likewise asymptotic. This means that if S is a series that asymptotically represents a function J (i.e with x large enough and ϵ however small that

   $$|\mathrm{J} - \mathrm{S_n}| < \frac{\epsilon}{x^n})$$

   it follows that

   $$\left| \int_x^\infty \mathrm{J}\ dx - \int_x^\infty \mathrm{S_n}\ dx \right| < \frac{\epsilon}{(n-1)x^{n-1}}.$$

Poincaré then stated that it was, in general, not permissible to differentiate an asymptotic series. As far as I know, this is the first time that the allowable algebra with asymptotic expansions were clearly stated with proof.

An important remark followed regarding the uniqueness of asymptotic expansions. Up until this point in the paper, it was implied that when x grew indefinitely that meant increasing by positive real values. However, Poincaré claimed that the theory was not changed when x tended to infinity with an argument, θ in $x = re^{i\theta}$, other than zero. There are, however, some important considerations here which Poincaré stated:

1. For a given function J and a given argument of x, there is only one asymptotic expansion representation of J.

2. For a given function J, if x tends to infinity with different arguments, the asymptotic expansion of J depends on the argument of x. This is Stokes phenomenon where the asymptotic behaviour of a function can differ in different regions of the complex plane.

3. A given asymptotic expansion can represent more than one function.

Poincaré said:

> "Je dis en effet que:
> $$x^2\left(J - A_0 - \frac{A_1}{x} - \frac{A_2}{x^2}\right)$$
> ne peut pas tendre vers 0 pour tous les arguments de x (ou du moins ne peut pas tendre uniformément vers 0), sans quoi J serait une fonction holomorphe de $\frac{1}{x}$ et la série serait convergente."[4] (Poincaré 1886, p.8)

Poincaré gave a basic example to show that, for the same argument of x, a given series can represent several different functions asymptotically. Moreover, for a given argument of x, a given function can only be represented asymptotically by a single series.

The second part of the 1886 paper used the mathematics developed in first part to analyze the results obtained by Lazarus Immanuel Fuchs (1833-1902) and Wilhelm Ludwig Thomé (1841-1910) for solutions to the linear differential equation

$$P_n\frac{d^ny}{dx^n} + P_{n-1}\frac{d^{n-1}y}{dx^{n-1}} + \ldots + P_1\frac{dy}{dx} + P_0y = 0$$

where the coefficients P_i are integer polynomials in x of degree i.

There are n independent series solutions that satisfy this differential equation of order n, if the degree of the polynomials $P_n, P_{n-1}, \ldots, P_0$ steadily decreases, meaning that the degree of P_{i-1} is always less than the degree of P_i. These solutions all converge for $|x|$ large enough. Poincaré, however, wanted to consider the series solutions

$$x^\alpha\left(A_0 + \frac{A_1}{x} + \frac{A_0}{x^2} + \ldots\right)$$

where α was given by some determining equation and $|x|$ was large, but the condition on the degree of the polynomials was removed such that the solutions that formally satisfy the linear differential equation became:

[4] I say in effect that: $x^2\left(J - A_0 - \frac{A_1}{x} - \frac{A_2}{x^2}\right)$ cannot tend to 0 for all arguments of x (or at least cannot tend uniformly to 0), otherwise J would be a holomorphic function of $\frac{1}{x}$ and the series would be convergent.

$$e^Q x^a \left(A_0 + \frac{A_1}{x} + \frac{A_2}{x^2} + \dots\right)$$

where Q was an integer polynomial in x. These series are not always convergent.

The remainder of the paper analyzed these solutions under various conditions and concluded that the most general integral of an equation was represented asymptotically by one of the normal series which formally satisfies this same equation. It is this mathematics from *Sur Les Intégrales irrégulières des équations linéaires* that was repeatedly referred to in the three body memoir and was used in the *New Methods of Celestial Mechanics*.

Schlissel claimed that it is this work that established asymptotic expansions to solutions of differential equations as a distinctive branch of mathematics. Schlissel said:

> "The theory of asymptotic series solutions of differential equations became a distinctive branch of mathematics with the contributions of one of France's most eminent mathematicians Henri Poincaré(1854-1912). His introduction of the theory of asymptotic series replaced the vague 'approximate solution' with the precise notion of 'asymptotic solution'." (Schlissel 1977, p.321)

I claim that the work of Poincaré did this, as we shall see below, and did more than this. It also provided methods and justification for obtaining asymptotic expansions to convergent infinite series regardless of whether or not those infinite series were formal solutions of a differential equation.

I used the example of Erdélyi's 1954 text earlier to show that the language of Poincaré is now typically used. In 1965, the British mathematician Edward Thomas Copson (1901-1980) wrote *Asympotic Expansions* (Copson 1965) in which he acknowledged the advice and help of Erdélyi. This text was written at the request of the admiralty and expanded upon a shorter work written in 1943. Perhaps not coincidentally, Erdélyi's work, as with a vast amount of mathematical research in those days, was supported by the Office of Naval Research in the United States. I expand on this point in the conclusion.

Copson's assessment of Poincaré's work is broader. He claimed that Poincaré had the most influence on the mathematics of divergent series that later developed. Copson said:

> "The modern theory of asymptotic expansions originated in the work of Poincaré. The subject falls roughly into two parts. The first part deals with the summability of asymptotic series, and with the validity of such operations as term by term differentiation or integration; the second is concerned with the actual construction of a series which represents a given function asymptotically. This

> tract discusses the asymptotic representation of a function defined by a definite integral or contour integral, usually an analytic function of a complex variable z." (Copson 1965, p.3)

Copson further commented on the importance of Poincaré's work specifically with regard to the finding of approximate solutions to differential equations. Copson claimed:

> "Poincaré's theory of asymptotic series had a profound effect on the study of approximate solutions of differential equations. The vague concept of approximate solution was replaced by the well defined asymptotic solution. Henceforth approximate solutions would be considered within the framework of Poincaré's theory of asymptotic series." (Copson 1965, p.329)

In the conclusion, I will again use the work of Whittaker, Copson and Erdélyi to establish that Poincaré's definitions were used in the textbooks that followed. It appears that the work of Stokes did not influence the later development of the mathematics of asymptotic expansions. Prior to Poincaré, clear definitions regarding asymptotic expansion were lacking, and statements and proofs of the allowed algebra of asymptotic expansions were also lacking. Poincaré, in 1886, clearly and carefully provided theory that he himself used only a few years later. He applied his theory to the equations of celestial mechanics to win a major prize and then built upon those results in the enormously influential *New Methods in Celestial Mechanics*. Subsequent to 1886, asymptotic expansions, improved and refined perhaps, became part of standard texts of mathematical practice. They were built on the work of Poincaré.

This chapter began with a discussion of the prize competition and the three body problem. Part of the discussion concerned the stability of the solar system and part of answering that question required an understanding of the behaviour of the infinite series involved. During the time in which Poincaré wrote and then refined his prize competition entry, he also wrote an article for a general audience in which he reflected on stability of the solar system. In that article, he brought forth physical arguments which he claimed have a large enough impact on the long term stability of the solar system that the mathematical approximations were not a determining factor.

I conclude this chapter with a short commentary based on Poincaré's 1898 *Nature* article titled *On the Stability of the Solar System* (Poincaré 1898), which is a translation of an article originally published in the *Annuaire du Bureau des Longitudes*. In this article, Poincaré first said that many people had demonstrated the stability of the solar system and that various physical effects that might affect the long term stability of the solar system had been studied and found to be not consequential. He did, however, point out that each demonstration was a successive approximation and that it is possible that future rigorous reasoning could show that the solar system was, in fact, unstable. Poincaré provided the results of his earlier investigations and stated that he had shown that, in particular cases, the orbit of one planet will return an infinite number of times to very nearly its initial position.

The majority of the rest of the article was a reflection on what Poincaré felt would the final resting position of the celestial bodies of our solar system. This discussion was based on an energy use or entropy argument. Poincaré claimed that, over a very long time frame, things like the energy loss due to tidal movements among other forces, which Poincaré called complementary forces, outweigh other considerations. Those other considerations included mathematical approximations of which the neglecting of terms when using series expansions was one example. Poincaré said:

> "... without quoting figures, I think that the effects of these complementary forces are much greater than those of the terms neglected by the analysts in the most recent demonstrations on stability." (Poincaré 1898, p.184)

Poincaré went on to conclude that that the celestial bodies are very slowly tending toward a state of final repose and stated again that neglected terms in the stability demonstrations were inconsequential. In the final paragraph of *On the Stability of the Solar System*, Poincaré said:

> "Thus the celestial bodies do not escape Carnot's law, according to which the world tends to a state of final repose. They would not escape it, even if they were separated by an absolute vacuum. Their energy is dissipated; and although this dissipation only takes place extremely slowly, it is sufficiently rapid that one need not consider terms neglected in the actual demonstrations of the stability of the solar system." (Poincaré 1898, p.185)

We have now seen two episodes in the mid to late nineteenth century where asymptotic expansions were used — first by Stokes in 1848 and again by Poincaré in 1886. Both of these episodes were motivated by a physical problem — for Stokes the problem originated in hydrodynamics and for Poincaré the problem came from celestial mechanics. We have also seen that the work of Poincaré was oriented and presented very differently from that of Stokes. Poincaré's work was much more 'mathematical' whereas the works of Stokes was more pragmatic.

This difference in the presentation of the work of Poincaré and Stokes was, in part, a reflection of the differing mathematical practices of the French and the English as discussed earlier in Chapter 4. However, given that the work of Poincaré was forty years after the work of Stokes, the changing standards of rigour during this time almost certainly played a role as well.

The process of rigorisation of analysis during the nineteenth century was motivated by a variety of factors. New technical developments, of which Fourier series is a particularly important example, made it necessary to examine the concepts of limit, function, convergence, and continuity more closely. The separation of mathematics from physics, and the separation of analysis from geometry, removed two previous foundational justifications for analysis, which then required replacement. Teaching was also a motivating force for clarification of the foundations of analysis; Cauchy, Weierstrass, and Dedekind were all motivated to examine foundational issues while preparing to lecture or while authoring textbooks.

The rigorous foundations of analysis recognized today developed primarily in two places and the development was dominated by two people. Cauchy, in France, played the major role in the first half of the nineteenth century and Weierstrass, in Germany, played the major role in the second half of the nineteenth century resulting in a satisfactory foundation for analysis by the beginnings of the twentieth century. These developments in analysis are well explained in (Lützen 2003) and (Archibald 2007).

In the Lützen chapter of *A History of Analysis* (Jahnke 2003), I note that there is no mention of any British mathematicians. The uptake of rigorous mathematical analysis happened later in England then on the continent and that change was fomented, in part, by Hardy via his influential 1907 textbook, *A Course of Pure Mathematics* (Hardy 1908). For example, historian Ivor Grattan-Guinness concluded that, with some exceptions that occurred during the mid-nineteenth century, that rigorous standards of analysis came to Britain early in the twentieth century. He said:

> "The Continental Analysis did not make much impact in Britain until the early 1840s, when William Thompson, later to become Lord Kelvin but then still a teenager, began to study Fourier series and integrals. Even then British interest lay chiefly in applications to mathematical physics, where the achievements were very brilliant, rather than in foundations ... Not until the work in the early years of this century [the 20th] by G.H. Hardy and W.H. Young were foundational studies brought fully into British education and research." (Grattan-Guinness 2000, p.31)

Thus, part of the difference seen between the work of Stokes and Poincaré is a result of the change in standards of analysis in the time between Stokes and Poincaré. This is in addition to the difference resulting from the culture in which the work was done. One salient difference is that Poincaré clearly had uniform convergence and he used the term absolutely convergent as well. All of the language in Poincaré's work sounds modern in contrast to that of Stokes and Poincaré was writing after Weierstrass, of whose work he was well aware, so there was no vague language involving limits.

Further the role of analysis changed substantially over the course of the nineteenth century such that at the end of the nineteenth century all physical theories were stated in mathematical terms. At the beginning of the century, by contrast, mathematical methods were limited primarily to celestial astronomy (Archibald 2003).

Next, and finally, I examine the 1886 doctoral dissertation of Stieltjes — a piece of mathematics, unrelated to any physical problem, which examined how to efficiently compute approximate values for several specific, and possibly divergent, infinite series.

Chapter 6

Stieltjes and Semi-convergent Series

Thomas Jan Stieltjes (1856-1894) was a Dutch mathematician who is now well-known for his work which founded the analytic theory of continued fractions. This was the context in which he introduced the Riemann-Stieltjes integral. This best known work of Stieltjes, from the early 1890s, interfaced with divergent series in a manner that was a precursor to summability theory. In this thesis, I omit discussion about the relationship between divergent series and summability theory and focus on the asymptotic expansions of divergent series. Prior to 1890, Stieltjes considered asymptotic expansions of divergent series. This was the topic of his doctoral thesis and the work that I focus on here.

Stieltjes used the term semi-convergent rather than asymptotic for these series and, in this chapter, I use these two terms interchangeably. Kline (Kline 1990) claimed that the term semi-convergent was used throughout the nineteenth century after being introduced by Legendre in 1798 in his book *Essai de la théorie des nombres* and that the term also applied to oscillating series. Divergent oscillating series do not converge but their partial sums stay bounded. A second edition of Legendre's book was published in 1808 and was reprinted by Cambridge University Press in 2009 (Legendre 2009). As seen in the introduction of this thesis, Legendre was interested in the distribution of the prime numbers amongst the positive integers.

Ferraro (Ferraro 2007b) claimed that Legendre knew the divergent and asymptotic nature of the series he used and Legendre termed them semi-convergent because "they first decrease (converge, in the language of his time) and then increase (diverge)" (Ferraro 2007b, p.474). Here the term semi-convergent will always mean the same as asymptotic.

Stieltjes, who worked briefly as an assistant at Leiden University, decided to pursue mathematical studies after corresponding with Charles Hermite (1822-1901) about celestial mechanics. Hermite, along with Darboux, later became Stieltjes' doctoral advisor at the suggestion of the two Frenchmen.

Stieltjes had an extensive correspondence with Hermite both before and subsequent to his doctoral thesis. This correspondence was published in 1905 in two volumes (Hermite & Stieltjes 1905a) and (Hermite & Stieltjes 1905b). In a February 13, 1886 letter, Stieltjes wrote to Hermite about his thesis topic. He told Hermite that he had abandoned his original topic because, though he felt he had some insight into it, there was too much more work needed. Stieltjes said:

> "Je travaille à ma Thèse Étude de quelques séries semi-convergentes, en deux mois j'espère l'avoir finie. Vous voyez, par là, que j'ai abandonné ma première idée. En effet, d'un côté, j'étais peu content de certaines parties et, de plus, j'avais vu que le sujet comporte encore de grands développements que j'entrevois un peu, mais qui demandent encore beaucoup de travail."[1] (Hermite & Stieltjes 1905a, p.180)

Shortly after he informed Hermite of the change of thesis topic, Stieltjes told Hermite that much of his thesis may not be of much interest to Hermite but that his idea for a semi-convergent series for $\Gamma(ai)$, with a real, may be of interest. Stieltjes said:

> "Ma Thèse contiendra beaucoup de choses qui vous intéresseront bien peu. Ce qui vous plaira peut-être le mieux, c'est que j'ai l'idée d'une série semi-convergente pour $\Gamma(ai)$ a réel très grand ou plutôt de $\log \Gamma(ai)$."[2] (Hermite & Stieltjes 1905a, p.181)

In the 1886 letter, Stieltjes further explained how he started with Gauss' definition of $\Gamma(ai)$ and developed that into a semi-convergent expansion. The work detailed in this letter was replicated with very little change in his thesis as we shall see. Stieltjes concluded that what he had done was so simple that he could not believe it was anything new.

[1]I am working on my Thesis Study of some semi-convergent series, in two months I hope to have finished it. You see, by that, that I have abandoned my first idea. Indeed, on the one hand, I was not happy with certain parts and, moreover, I had seen that the subject still includes great developments which I foresee a little, but which still require a lot of work.

[2]My Thesis will contain many things which will interest you very little. What you may like best is that I have the idea of a semi-convergent series for $\Gamma(ai)$ a very large real or rather $\log \Gamma(ai)$

Earlier letters, from July of 1885, indicate that Stieltjes had likely originally considered a thesis topic investigating the Riemann zeta function. In a letter from July 29, 1885, which followed correspondence about the zeros of the zeta function, Hermite informed Stieltjes that he and Darboux were interested in him obtaining the title of Doctor which would allow Stieltjes to become a professor in a provincial faculty of science until a position in Paris was arranged (Hermite & Stieltjes 1905a, p.165). On August 28, 1885, in his next letter to Hermite, Stieltjes responded very enthusiastically to the suggestion that he obtain a doctorate and indicated that Hermite would have already found out through Darboux that he had accepted the proposition. The remainder of this letter was about elliptic functions and properties of the Riemann zeta function.

Stieltjes' thesis was published after he had already obtained a professorship, in 1884, at the University of Leiden. The professorship was made possible by the intervention of Hermite who helped get Stieltjes awarded an honorary doctorate after the University of Groningen declined his well supported application due to a lack of a diploma. In 1889, Stieltjes accepted a position at the University of Toulouse where he remained until his death.

Stieltjes' doctoral thesis was much later in his career than is typical now. Stieltjes already had a professorship, was already well-known internationally, and was already a published mathematician when his thesis was published. His thesis resembles a paper as it does not look substantially different from the papers that came before it.

There was no mention of the divergent series of celestial mechanics in Stieltjes' doctoral thesis despite the initial interest in celestial mechanics that precipitated Stieltjes' mathematical studies. As we shall see, some of the examples he used in his thesis are from number theory. Here then is another and different motivation for the study of asymptotic expansions of divergent series.

For basic biographical information and an analysis and summary of Stieltjes' scientific work see the introduction to the annotated and republished collected works of Stieltjes, edited by Gerrit van Dijk and published in 1993 to commemorate the one hundredth anniversary of the death of Stieltjes (Stieltjes & van Dijk (ed.) 1993a).

6.1 Stieltjes' 1884 Paper on Continued Fractions

Just prior to the publication of his thesis, Stieltjes authored, in 1884, a first paper on continued fractions titled *Sur un développement en fraction continue* (Stieltjes 1884). In this paper he proved the convergence of the continued fraction

$$\cfrac{2}{z - \cfrac{\frac{1\cdot 1}{1\cdot 3}}{z - \cfrac{\frac{3\cdot 3}{5\cdot 7}}{z - \cfrac{\frac{4\cdot 4}{7\cdot 9}}{z - \cdots}}}}$$

in the slit complex z-plane excluding the interval $(-1, 1)$. To do this Stieltjes started with the approximation, via Gaussian quadrature, of the integral

$$\int_{-1}^{+1} F(x)dx \approx A_1 \text{ F}(x_1) + A_2 \text{ F}(x_2) + \ldots + \text{A}_n \text{ F}(x_n)$$

where $F(x)$ was not specified. He had shown in a previous memoir (Stieltjes 1887) that

$$-1 < x_1 < -1 + \text{A}_1 < x_2 < -1 + \text{A}_1 + \text{A}_2 < x_3 < \ldots < -1 + \text{A}_1 + \ldots + \text{A}_{n-1} < x_n < +1.$$

Gaussian quadrature is a method of numeric integration in which a definite integral, between the limits of -1 and 1, is approximated with a weighted sum of carefully selected function evaluations of the integrand. There are several types of Gaussian quadrature and the type that Stieltjes used, now called Gaussian-Legendre quadrature, required the integrand to be evaluated at the (always real) zeros of the n^{th} order Legendre orthogonal polynomials normalized such that $P_n(1) = 1$. Thus it is immediately apparent why all of the x_i's, called nodes, are between -1 and 1. Further, for this selection of nodes, the correct weighting (here A_i) is computed from the nodes and the derivative of $P_n(x)$ and always results in weights that are positive and less than one.

The only restriction on the integrand, $F(x)$, is that it must be possible to evaluate it at the nodes. The Gaussian-Legendre quadrature rule will only be an accurate approximation to the integral if $F(x)$ is well-approximated by a polynomial of degree $2n - 1$ or less on $[-1, 1]$. The approximation is exact when the integrand, $F(x)$, is any polynomial of degree $2n - 1$ or less.

A series of approximations to a continued fraction can be found by terminating the continued fraction after n terms and simply evaluating the resulting finite fraction. This is called the n^{th} order approximate of the continued fraction which Stieltjes denoted $\frac{\text{P}_n}{\text{Q}_n}$. He then considered the partial fraction decomposition of the approximate

$$\frac{\mathrm{P}_n}{\mathrm{Q}_n} = \frac{\mathrm{A}_1}{z - x_1} + \frac{\mathrm{A}_2}{z - x_2} + \cdots + \frac{\mathrm{A}_n}{z - x_n}$$

where the A_i and x_i are the same as for the Gaussian quadrature approximation of the definite integral from -1 to 1 of $F(x)$. The A_i and x_i are therefore all bounded between -1 and 1. Since the x_i appear as roots of Q_n, z was excluded from taking any values in the interval $(1, -1)$. The partial function decomposition is an infinite series as n tends to infinity and in the limit as n tends to infinity the sum of the partial fractions can be represented as an integral such that

$$\lim_{n\to\infty} \frac{\mathrm{P}_n}{\mathrm{Q}_n} = \int_{-1}^{+1} \frac{dx}{z - x}.$$

This is a convergent definite integral when z does not take any value between -1 and 1. Therefore the continued fraction converges with the same restriction on z. Stieltjes relied on the A_i's and x_i's being in the interval $(-1, 1)$ and that they came from the Gaussian approximation to the definite integral to claim that the infinite series he used was convergent.

Stieltjes published, in 1883 and 1884, work on which the above analysis was based. This is considered his early work on moments (Fischer 2010, p.160) in which he approached the moment problem through a discussion of Gaussian quadrature by means of continued fractions. The Stieltjes moment problem, in modern terminology, is to find the necessary and sufficient conditions for a sequence, $m_0, m_1, m_2, ...$ to be of the form

$$m_n = \int_0^\infty x^n d\mu(x)$$

for some measure μ.

By 1883 Stieltjes had shown that, for an arbitrary positive weight function, f, the Gaussian quadrature formula for

$$\int_a^b g(x) f(x) dx$$

converged to g when g was a uniformly convergent series of Legendre polynomials. The proof was connected to the analysis of the continued fraction

$$\cfrac{M_0}{x - \alpha_0 - \cfrac{\lambda_1}{x - \alpha_1 - \cfrac{\lambda_2}{x - \alpha_2 - \cfrac{\lambda_3}{x - \alpha_3 - \cdots}}}}$$

which was associated with the definite integral

$$\int_a^b \frac{f(z)}{x-z}\, dz.$$

Stieltjes later investigated the continued fraction

$$\cfrac{1}{a_1 z + \cfrac{1}{a_2 + \cfrac{1}{a_3 z + \cfrac{1}{a_4 + \cdots}}}}$$

in work which was published in 1894 just before his death. This is considered his most important work and was titled *Recherches sur les fractions continues* (Stieltjes 1894). Again, Stieltjes considered the roots of the polynomials of the sequence of approximates to the continued fraction and formed a partial fraction decomposition which he was able to use to manipulate the continued fraction into an infinite series in decreasing powers of z,

$$\sum_{k=1}^{\infty}(-1)^{k-1}\frac{C_{k-1}}{z^k}.$$

From this he obtained a related infinite series which he used to show that when that infinite series diverged, the related continued fraction converged (Bernkopf 2008, p.57). This was an interesting use of a divergent series in a manner that does not require an expansion or a sum. It was somewhat akin to what Leibniz did regarding the $\log(-1)$ discussed in the introduction. The convergence or divergence of an infinite series was used to say something about value or the convergence of something else.

Perhaps part of Stieltjes' interest in semi-convergent series as a thesis topic was a result of his investigations in continued fractions prior to his thesis. Certainly Stieltjes continued to use semi-convergent series subsequent to the thesis and they were useful to him in his important later investigations into continued fractions.

6.2 Stieltjes' 1886 Doctoral Thesis

In June of 1886, Stieltjes' doctoral thesis was published in the *Annales scientifiques de l'École normale supérieure*. The title of the thesis was *Recherches sur quelques séries semi-convergentes* (Stieltjes 1886) and it was 58 pages long and contained neither footnotes nor a bibliography.

Stieltjes stated the scope and intent of his thesis in an eight page introduction. Stieltjes' goal for the thesis was to study several semi-convergent series where the variable was exclusively real and positive. He kept to a real, positive variable which he consistently called a. However, he spent a significant portion of the thesis examining both the semi-convergent expansion of $\Gamma(ai)$ and $\mathrm{J}(ai)$ where a was real and positive and $\mathrm{J}(ai)$ is a Bessel function. Thus he considered a semi-convergent expansion for a function with a purely imaginary argument, say b, where $b = ai$.

Stieltjes divided the series he investigated into two kinds. Series of the first kind were alternating series and series of the second kind had all terms of the same sign. This was an important distinction. For series of the first kind the exact value of the function is always between the sum of the first n and $n+1$ terms of the series even though this value may not converge to any particular limit. It is much more difficult to bound the error in the approximation of a function for infinite series of the second kind. Stieltjes called both types of series semi-convergent.

Stieltjes placed little emphasis on series of the first kind after he showed how an error bound was relatively easily obtained for these types of semi-convergent series. He then investigated five particular examples of semi-convergent series of the second kind, each in its own section. Those section headings and lengths are:

1. Study of the logarithmic integral (11 pages),
2. Study of the integrals $\int_0^\infty \frac{\sin au}{1+u^2}du, \int_0^\infty \frac{u\cos au}{1+u^2}du$ (5 pages),
3. Semi-convergent series expansion of $\log\Gamma(ai)$ (12 pages),
4. Study of the integrals of the differential equation $\frac{d^2z}{da^2} + \frac{1}{a}\frac{dz}{da} + z = 0$ (22 pages),
5. Study of the function $\mathrm{P}(a) = \sum_1^\infty \frac{1}{e^{\frac{n}{a}} - 1}$ (7 pages).

Stieltjes claimed that the treatment of series of the second kind in the mathematical literature were quite rare and that he had only encountered one — a series due to Oskar

Schlömilch (1823-1901). This is a somewhat surprising statement since Stieltjes had an interest in celestial mechanics where divergent series were contemporaneously being used. However, he was likely unaware that Poincaré had proved that some of the series used in celestial mechanics were divergent and had provided asymptotic expansions for them since that work was published essentially concurrently.

There are other reasons to believe Stieltjes had likely seen treatment of series of the second kind previously. First, he mentioned the Stirling series in his thesis. Also, in 1890, Stieltjes published a paper titled *Sur la valeur asymptotique des polynômes de Legendre* (Stieltjes 1890) on the asymptotic behaviour of the Legendre polynomials. This built upon the 1878 work of Darboux. While Stieltjes' 1890 paper was four years after the doctoral thesis, Darboux was the co-supervisor for Stieltjes' thesis. It is likely that Stieltjes would have known that, in an 1878 paper, Darboux gave an asymptotic series for the Legendre polynomials (Darboux 1878) which generalized an asymptotic formula given by Laplace — this 1790 asymptotic expansion of Laplace is mentioned in the introduction along with Laplace's 1812 asymptotic expansion of the error function.

When considering the asymptotic behaviour of the Legendre polynomials there are two fundamental problems — the asymptotic behaviour of the polynomials outside the interval of orthogonality and the asymptotic behaviour inside the orthogonality interval. The second problem is more difficult (Szegö 1939, p.195) and it is the problem that Laplace, Darboux and Stieltjes solved with an increasing level of sophistication. For these three expansions, see (Szegö 1939, p.194-195). These expansions, typically written as $P_n(\cos\theta)$ make clear that the argument for which the expansion is being used is between -1 and 1. The Legendre polynomials were the polynomials Stieltjes used in Gaussian quadrature.

Darboux's semi-convergent series for the Legendre polynomials was, however, a poor example of an asymptotic expansion, partly because there was no closed expression or bound on the error term. Moreover, Darboux's semi-convergent infinite series was asymptotic for some values of the variable and was convergent for other values of the variable. For the values of the variable where the semi-convergent series was convergent, the series did not converge to the Legendre polynomial it approximated and thus it was an example of an asymptotic expansion that did not converge to the function that it approximated. According to Van Assche, the reason for this was that the method of Darboux did not work correctly. Darboux's method consisted of using the singularities, of which there are two, on the circle of convergence of the generating function of the Legendre polynomials. Each singularity gave information about the polynomial with the result that the infinite series converged to a function that was twice the one being approximated. VanAssche said:

> "The reason why things go wrong here is that the formula is obtained by the so-called method of Darboux which consists of obtaining asymptotic results of a sequence by carefully examining the singularities on the circle of convergence of the generating function. The generating function of Legendre polynomials has two singularities on the circle of convergence, and at each singularity one picks up information on $P_n(\cos\theta)$. This is probably the reason why the convergence of the infinite series is to $2P_n(\cos\theta)$ rather than $P_n(\cos\theta)$. Stieltjes' generalization of Laplace's asymptotic formula for the Legendre polynomials does not suffer from either problem." (Van Assche 1993, p.24)

Stieltjes, in 1890, was able to generalize Laplace's asymptotic formula for the Legendre polynomials and provide an expansion where the error term was boundable and the expansion converged, in the ordinary sense, to the function, $\mathrm{P}_n(\cos(\theta))$, it approximated. This, when combined another asymptotic formula that related the Legendre polynomial asymptotic expansion to the Bessel function of order zero, allowed Stieltjes to find an asymptotic series for the Bessel function, J_0, already obtained by Poisson, but now with an error bound (Van Assche 1993, p.25). It is hard to imagine that Stieltjes was unaware of these various other examples of semi-convergent series when he made the statement that he was only aware in the literature of the series of Schlömilch. Perhaps Stieltjes felt that the Schlömilch example was the only one in the literature that was mathematically rigorous enough.

In the remainder of this chapter, I look first at the Schlömilch semi-convergent expansion because that provides an overview of Stieltjes' method. Then I provide a general discussion of the method of Stieltjes and follow that with a detailed analysis of the Stieltjes method in the context of finding a semi-convergent expansion to the logarithmic integral. Finally I consider the remaining examples of the thesis, in less detail, in order to clarify what Stieltjes accomplished in his thesis. Later we shall see that, in an 1890 paper, Stieltjes built upon the work he did with the Schlömilch example in his dissertation.

6.2.1 The Schlömilch Semi-convergent Expansion

I claim that Stieltjes analyzed the Schlömilch semi-convergent expansion for J_∞ in his thesis partially because it allowed him, as it did Schlömilch, to start with an integral expression for the n^{th} order Bessel function of the first kind and then find a semi-convergent expansion for J_∞. Stieltjes, with his new method, found the same semi-convergent series as Schlömilch which provided Stieltjes with confidence in his method. Stieltjes was also able to provide a better error bound than Schlömilch.

The Schlömilch example is from an 1861 paper, published in *Zeitschrift für Mathematik und Physik*, a journal of which Schlömilch was the founder and an editor, and concerned the semi-convergent series representing the function:

$$\mathrm{P}(a) = \sum_{n=1}^{\infty} \frac{1}{e^{\frac{n}{a}} - 1}$$

This was the last of the five examples that Stieltjes analyzed in his thesis and, after some discussion here, we return to it later.

He first summarized the results obtained by Schlömilch — a remarkable semi-convergent series according to Stieltjes:

$$\mathrm{J}_\infty = \frac{B_1^2}{2 \cdot 2!a} + \frac{B_2^2}{4 \cdot 4!a^3} + \ldots + \frac{B_n^2}{2n \cdot (2n)!a^{2n-1}} + R_n$$

where the B_i are the Bernoulli numbers. Schlömilch initially provided a bound on R_n of $\frac{\pi}{2} T_{n+1}$ where T_{n+1} is the first term omitted. As Stieltjes pointed out, Schlömilch noted that this bound was not the best possible and returned to his analysis and provided a better bound of

$$\frac{2nB_{n+1}}{B_n} \frac{1}{a} \left(\frac{\pi^2}{6} + \frac{1}{4\pi^2 a^2} \right) T_n$$

Stieltjes then discussed this series and the bound on the remainder term from his point of view. To do that he used the formula:

$$\mathrm{J}_\infty = \left(p.v. \int_0^\infty \frac{4avdv}{1 - v^2} \right) P \left(\frac{1}{4\pi^2 av} \right)$$

where $p.v.$ denoted the Cauchy principal value of the integral. $\mathrm{P}(a)$ is the original function for which a semi-convergent expansion was desired and Stieltjes had showed previously that

$$\mathrm{P}(a) = a(\log a + \Theta) + \frac{1}{4} - \mathrm{J}_\infty$$

After he showed this representation of J_∞ was equivalent to the semi-convergent series of Schlömilch, Stieltjes used the method of finding the zero of R_n, which will be explained below, and gave a simpler and better approximation of R_n of

$$2\pi^2 a - \frac{5}{12}$$

Stieltjes evaluated $\mathrm{P}\left(\frac{1}{4}\right)$ using his semi-convergent series to get $\mathrm{P}\left(\frac{1}{4}\right) = 0.0189994$ where the exact value is 0.0189992. From this he concluded that the approximation was excellent. Stieltjes said:

> "L'approximation avec laquelle nous avons résolu l'équation $R_n = 0$ ne laisse rien à désirer."[3] (Stieltjes & van Dijk (ed.) 1993a, p.258)

I now turn to a general discussion of Stieltjes' method before returning to the remaining four examples.

6.2.2 The Stieltjes Method of Semi-convergent Expansion

Stieltjes defined his terms and explained his method before he developed his five examples. He then used his method on a first, simple example for which he gave a careful detailed explanation. This example was the asymptotic expansion of the logarithmic integral.

In this section, I briefly discuss the method of Stieltjes in general terms. In the following section, I use the Stieltjes study of the logarithmic integral to demonstrate how his method worked on a specific example. I do not discuss the other examples in the Stieltjes thesis in detail but summarize them to bring forth some of the important features of Stieltjes' work.

Stieltjes considered a divergent series representation of the function $\mathrm{F}(a)$,

$$\mathrm{F}(a) = m_0 + \frac{m_1}{a} + \frac{m_2}{a^2} + \frac{m_3}{a^3} + \ldots$$

which he said cannot be used indefinitely for numerical calculation because the series is divergent. He then immediately said that the series representation of $\mathrm{F}(a)$ must be considered symbolically which meant that, in the limit as a approached infinity:

$$\begin{aligned} &\lim \mathrm{F}(a) = m_0, \\ &\lim a\left[\mathrm{F}(a) - m_0\right] = m_1, \\ &\lim a^2\left[\mathrm{F}(a) - m_0 - \frac{m_1}{a}\right] = m_2. \end{aligned}$$

Should it be desired to use the divergent series to evaluate $\mathrm{F}(a)$, then that could only be done with certainty after a discussion of the remainder term left after using a finite number of terms. I interpret this to mean that symbolically meant as a formal series without regard to convergence.

[3]The approximation with which we solved the equation $R_n = 0$ leaves nothing to be desired.

This is a less sophisticated statement than that of Poincaré who defined S_n to be the asymptotic representation of function $\mathrm{J}(x)$ if the expression

$$x^n\,(\mathrm{J}-S_n)$$

tended to zero when x increased indefinitely. Stieltjes simply wrote down a divergent series and then claimed that for large values of the variable one could identify the constants in the series by recursively taking the above limits. However, Stieltjes observed that there are only a few cases where the coefficients m_0, m_1, m_2, ... follow a simple law which made finding large numbers of the constants impractical by Stieltjes' method of taking limits. This meant that his method did not work in general and there were only a few examples for which he method worked.

Next Stieltjes considered the terms of the series representation of $\mathrm{F}(a)$ and labelled them in the following manner:

$$\mathrm{F}(a) = T_1 + T_2 + ... + T_n + R_n$$

where he called R_n the complementary term; an expression I use interchangeably with remainder term.

If the terms were alternating in sign, then the series was called a semi-convergent series of the first kind and it was immediately possible to see that $\mathrm{F}(a)$ lay between $\sum_1^n T_n$ and $\sum_1^{n+1} T_n$. That, in turn, provided a bound on R_n as less in absolute value than T_n. If terms of the series were all of the same sign, then the series was a semi-convergent series of the second kind. While there was some commentary about series of the first kind, the thesis, in the main, was an attempt to study semi-convergent series of the second kind and say something about R_n. There are, of course, series which are not of the first or second kind in which the terms are positive and negative but do not alternate. Stieltjes made no mention of this possibility.

In semi-convergent series of the second kind, the terms decrease at first and then increase beyond all limit. If T_n is the smallest term in a series of the second kind, then R_n can exceed in absolute value any multiple of T_n. Thus R_n, if tied to T_n, will not provide the best error bound. Stieltjes said:

"donnera toujours des limites trop étendues et qui ne permettent point de tirer tout le parti possible de la série"[4] (Stieltjes & van Dijk (ed.) 1993a, p.203)

Stieltjes then introduced a key idea into the thesis. For series of the second kind, an error bound was determined by finding the first integer value of n larger than that for which the sign of the monotonic sequence R_n changed. For that value of n, the remainder term is bounded by T_n. This meant solving the transcendental equation $R_n = 0$ where n was now considered to be a continuous variable. Given that $R_n = 0$ is a transcendental equation, it was not possible to find the root algebraically and it was generally estimated by expanding the expression for R_n in its own semi-convergent expansion.

Stieltjes did not claim that the change of sign of R_n always happened in the neighbourhood of the smallest term, T_n, though he did say that this was desirable and that, in all the cases studied so far, this had always been the case. There is only one root to $R_n = 0$ due to the monotonicity of the sequence and the value of n was approximated by another semi-convergent series of the form

$$n = \alpha a + \alpha_0 + \frac{\alpha_1}{a} + \frac{\alpha_2}{a^2} + \dots$$

which was inverted to give

$$a = \beta n + \beta_0 + \frac{\beta_1}{n} + \frac{\beta_2}{n^2} + \dots$$

As Stieltjes admitted, it so far does not look like progress has been made but if only a weak approximation to $R_n = 0$ was required then it was possible to numerically compute using the two series above attributing much smaller values to a and n than were used in actually approximating $\mathrm{F}(a)$. Stieltjes claimed that the approximation thus obtained for n was generally sufficient.

In light of this analysis of the error bound for series of the second kind, Stieltjes briefly returned to semi-convergent series of the first kind where finding the solution to $R_n = 0$ was replaced with finding the solution of the also transcendental equation $\dfrac{dR_n}{dn} = 0$. The solution to this equation was approximated by $R_{n-1} = R_n$ from which straightforward analysis gave $\lim_{n\to\infty} \dfrac{R_n}{T_n} = \dfrac{1}{2}$, a not surprising result given the alternating nature of the series of the first kind.

[4] will always give bounds that are too far apart and which do not allow us to derive all possible advantage from the series

The rest of the thesis considered series of the second kind which come up in a variety of contexts. These are the five examples listed above at the beginning of the chapter. I now consider the first of those examples in detail.

6.2.3 Finding the Semi-convergent Expansion of the Logarithmic Integral

Stieltjes stated that finding an asymptotic expansion to the logarithmic integral was a very simple example of a series of the second kind. He started with the definition of the logarithmic integral,

$$\mathrm{li}(a) = \int_0^a \frac{du}{\log u}$$

which is an improper integral when $a \geq 1$. Stieltjes was careful to make the definition of $\mathrm{li}(a)$ rigorous re-stating it as

$$\mathrm{li}(a) = \lim_{\varepsilon = 0} \left(\int_0^{1-\varepsilon} \frac{du}{\log u} + \int_{1+\varepsilon}^a \frac{du}{\log u} \right).$$

Stieltjes was interested in and published in number theory, including a failed proof of the Riemann hypothesis. His interest in the logarithmic integral may well have been because it provides a very good approximation to the prime counting function.

Stieltjes replaced the argument in $\mathrm{li}(a)$ with e^a and made a substitution in the variable of integration which gave

$$\mathrm{li}\left(e^a\right) = e^a \left(\int_0^{1-\varepsilon} \frac{e^{-av}}{1-v} dv + \int_{1+\varepsilon}^{\infty} \frac{e^{-av} dv}{1-v} \right)$$

Then the fraction $\dfrac{1}{1-v}$ in the integrals was replaced with the identity,

$$\frac{1}{1-v} = 1 + v + v^2 + \ldots + v^{n-1} + \frac{v^n}{1-v},$$

and the integral was integrated term by term which gave

$$\mathrm{li}\left(e^a\right) = e^a \left[\frac{1}{a} + \frac{1}{a^2} + \frac{1 \cdot 2}{a^3} + \ldots + \frac{1 \cdot 2 \ldots (n-1)}{a^n} + R_n \right]$$

and this is a semi-convergent series for $\mathrm{li}\left(e^a\right)$.

The geometric series only converges for values of $|v| < 1$ but there was no such restriction placed on v here making this identity, exact for finite values of n, a formal statement of equality when n approached infinity and when the absolute value of v was greater than or equal to 1.

The value of R_n, the remainder term, was

$$R_n = \int_0^{1-\varepsilon} \frac{v^n e^{-av}}{1-v} dv + \int_{1+\varepsilon}^{\infty} \frac{v^n e^{-av}}{1-v} dv$$

meaning that Stieltjes had obtained an asymptotic expansion of $\mathrm{li}(e^a)$ with an expression for the remainder term, R_n, where R_n was the Cauchy principal value of

$$\int_0^{\infty} \frac{v^n e^{-av}}{1-v} dv$$

Without evaluating this integral, Stieltjes determined that R_n always decreased as n increased so that, as promised, $R_n = 0$ had a single root, which Stieltjes proceeded to find. First a and n were coupled via $a = n + \eta$ where η was finite. Then a method of Laplace from the *Théorie analytique des probabilités* (Laplace 1812) was applied because it was applicable to evaluating integrals which contain functions raised to a high power. Consider a function, $f(x)$, twice continuously differentiable on the interval $[a, b]$ with a unique point $x_0 \in [a, b]$ where $f(x_0) = \max_{x_0 \in [a,b]} f(x)$ and $f''(x_0) < 0$. The method of Laplace states that, as $M \to \infty$,

$$\int_a^b e^{Mf(x)}\, dx \approx \sqrt{\frac{2\pi}{M|f''(x_0)|}} e^{Mf(x_0)}$$

Laplace's method can be used to derive Stirling's approximation from the definition of the gamma function (Wikipedia contributors 2023a).

Several pages of analysis followed in which Laplace's method was applied and some terms were neglected as they were small in comparison to the dominant terms. The end result of the computation was that R_n was found to be

$$\mathrm{R}_n = e^{-a}\sqrt{\frac{2\pi}{n}}\left[\eta - \frac{1}{3} + \left(\frac{1}{6}\eta^3 - \frac{1}{2}\eta^2 + \frac{1}{12}\eta + \frac{1}{540}\right)\frac{1}{n} + \left(\frac{1}{40}\eta^5 - \frac{5}{24}\eta^4 + \frac{25}{72}\eta^3 - \frac{1}{24}\eta^2 + \frac{1}{288}\eta + \frac{25}{6048}\right)\frac{1}{n^2} + \dots\right.$$

The importance of the relationship Stieltjes set up between a and n of $a = n + \eta$ is worth commenting on here. The only restriction placed on η was finiteness. It was thus a statement that a and n were related by some additive constant. This assumption was equivalent to assuming that the remainder term was to be computed at a value of n where T_n was at its smallest. Stieltjes said it thus:

> "Comme on suppose que η a une valeur finie, la supposition $a = n + \eta$ indique évidemment que nous considérons le reste d'un terme T_n dans le voisinage du plus petit terme."[5] (Stieltjes & van Dijk (ed.) 1993a, p.209)

Next Stieltjes set η to a semi-convergent expansion in powers of n which was then substituted into the expression for R_n. Because R_n was equated to zero, the coefficients of the various power of $\frac{1}{n}$ in the equation $R_n = 0$ were zero. That allowed Stieltjes to determine that:

$$a = n + \beta_0 + \frac{\beta_1}{n} + \frac{\beta_2}{n^2} + \ldots,$$

where the values of β were determined to be:

$$\beta_0 = +\frac{1}{3},$$
$$\beta_1 = +\frac{8}{405}$$
$$\beta_2 = -\frac{184}{25515}$$

From that he deduced that

$$n = a - \frac{1}{3} - \frac{8}{405a} + \frac{16}{25515a^2} - \cdots$$

These series for a and n are semi-convergent series of the first kind.

Let N be the approximate value of the root of $R_n = 0$. The complete result was summarized as follows:

$$\mathrm{li}\,(e^a) = e^a \left[\frac{1}{a} + \frac{1}{a^2} + \ldots + \frac{1 \cdot 2 \ldots (n-1)}{a^n} + \mathrm{R}_n\right]$$

with N an approximation formed from truncating the semi-convergent series for n giving

$$N = a - \frac{1}{3} - \frac{8}{405a} + \frac{16}{25515a^2}.$$

where the error in the approximation of $\mathrm{li}(e^a)$ was of order $\sqrt{\frac{2\pi}{a}}$.

The approximation in solving the equation $R_n = 0$ was judged empirically by comparison to the exact value. The value of n was first set to 1 and then to 2, and a comparison was

[5] As we assume that η has a finite value, the assumption $a = n + \eta$ obviously indicates that we consider the remainder term T_n in the neighbourhood of the smallest term.

n.	Valeur exacte.	Valeur approximative.	Erreur.
1............	1,3472	1,3459	0,0013
2............	2,34155	2,34141	0,00014

Figure 6.1: Comparison of Exact and Approximate Value of li(e^a) (Stieltjes & van Dijk (ed.) 1993a, p.214)

made to an exact value which was computed using the convergent series

$$\mathrm{li}(e^a) = \gamma + \log(a) + \frac{a}{1 \cdot 1!} + \frac{a^2}{2 \cdot 2!} + \frac{a^3}{3 \cdot 3!} + ...$$

with the results as shown in Figure 6.1. This showed Stieltjes that the calculation of the value of n needed in the approximation obtained by his method was much larger than necessary.

Stieltjes then computed the value of li(10000000000) and obtained 455055614.5866 using 23 terms of the asymptotic expansion which is the exact answer to four decimal places. In comparison, using the convergent series with 23 terms gave only the largest term. Thus it was necessary to make significantly more computations with the convergent series than were needed to find the approximation by the semi-convergent series for a comparable degree of accuracy.

Not content with only the above method for computing an error bound for the developed asymptotic expansion for li(e^a) above, Stieltjes found another way that, in this particular case, allowed a simple form to be obtained for the complementary term R_n. This form also gave an alternate method for relating n and a using the expansion

$$a = n + \beta_0 + \frac{\beta_1}{n} + \dots$$

Assume that $0 < b < a$ such that

$$\mathrm{li}\left(e^a\right) = \mathrm{li}\left(e^b\right) + \int_b^a \frac{e^u}{u} du,$$

and integrate by parts to get

$$\mathrm{li}\left(e^a\right) = e^a \left[\frac{1}{a} + \frac{1}{a^2} + \dots + \frac{1 \cdot 2 \dots (n-1)}{a^n} + R_n\right]$$

$$R_n = 1 \cdot 2 \cdot 3 \cdots n \cdot e^{-a} \int_{a_n}^a \frac{e^u}{u^{n+1}} du$$

where a_n needs to be determined but clearly it is the value of a that makes $R_n = 0$. By using the relationship $R_{n-1} = T_n + R_n$ and continuing with the computation of the values of $\beta_0, \beta_1, ...$, Stieltjes found exactly the same values of $\beta_0, \beta_1, ...$ that were obtained by the more generally applicable method first used.

The section of the thesis concerning a semi-convergent series for li(a) concluded with a brief remark that for values of a less than one, a semi-convergent series of the first kind was obtained instead. In that case, the bounding of the error in the expansion was straightforward and was of the order of the first term omitted.

6.2.4 Finding Semi-convergent Expansions

The method Stieltjes used to develop an asymptotic expansion for li(a), relied on a clever use of the geometric series. Thus it was not a general method. The method of finding the error bound was more general but, again, it relied on the use of a specialized technique of Laplace in order to evaluate the necessary integral.

Without going into as much detail as was done in the previous section, I will now comment on the similarities between the 5 examples chosen by Stieltjes and the way in which the asymptotic expansions and their error bounds were found. From this I conclude that what Stieltjes knew how to do in 1886 was to find asymptotic expansions of a limited number of functions for which he could find a way of simplifying and evaluating the resulting integrals.

Unlike the more sophisticated work done by Poincaré in the same year, in Stieltjes' thesis there was no discussion on what was mathematically possible with asymptotic expansions. Stieltjes did not address adding, multiplying, differentiating or integrating the expansions he found. He did, however, partially return to this the very next year in a paper titled *Note sur la multiplication de deux séries* (Stieltjes 1914, p.95).

The second example in the Stieltjes thesis was a study of the integrals:

$$\int_0^\infty \frac{\sin au}{1+u^2}du \qquad \text{and} \qquad \int_0^\infty \frac{u\cos au}{1+u^2}du$$

The method Stieltjes used to find an asymptotic expansion for these integrals started differently. The first integral was restated with a complex argument to give

$$\int_0^\infty \frac{e^{aui}}{1+u^2}du$$

which was evaluated on a contour consisting of a square in the first quadrant with one vertex at the origin. The integral along the x-axis is the same as integrating along the other three sides of the square with caution required at the point $(0, i)$. The contour at that point was deformed using a semicircle around that point with small radius ϵ. The integral containing the sine function is the imaginary part of this integral and the integral containing the cosine function is the derivative with respect to a of the integral with the sine function.

From this point, the method Stieltjes used for obtaining the expansion and the remainder term are exactly analogous to how he proceeded for $\operatorname{li}(a)$. Stieltjes used the geometric series identity with the variable now v^2 rather than v. This allowed him to find the asymptotic expansion and compute the remainder term in exactly the same manner so he omitted most of the details of the computations in the presentation of this example.

It is interesting that after finding both of the asymptotic expansions of these integrals via the method above, Stieltjes then used the partial fraction decomposition

$$\frac{1}{1-v^2} = \frac{1}{2}\left(\frac{1}{1-v}\right) + \frac{1}{2}\left(\frac{1}{1+v}\right)$$

to show that $\int_0^\infty \frac{\sin au}{1+u^2} du$ is related to the logarithmic integral in the following manner:

$$\int_0^\infty \frac{\sin au}{1+u^2} du = \frac{1}{2} e^{-a} \operatorname{li}\left(e^a\right) - \frac{1}{2} e^a \operatorname{li}\left(e^{-a}\right)$$

Further, he used known values of the logarithmic integral and the above relationship to compute the exact value of the integrals in order to see how good his semi-convergent approximations were. There was no attempt to simply use the asymptotic expansions of $\operatorname{li}\left(e^a\right)$ and combine them using arithmetic. Perhaps that is because this would have required an analysis of whether or not it is valid to do this type of arithmetic with semi-convergent series, a topic which he took up the following year.

Excepting the idea of taking the integrals $\int_0^\infty \frac{\sin au}{1+u^2} du$ and $\int_0^\infty \frac{u \cos au}{1+u^2} du$ and representing them as a portion of a contour integral in the complex plane, this example was a replication of the first example. The essential step in both cases was replacing

$$\frac{1}{1-x} = 1 + x + x^2 + \ldots + x^{n-1} + \frac{x^n}{1-x},$$

where in the first example $x = v$ and in the second example $x = v^2$. It was this step in both cases that allowed Stieltjes the ability to integrate term by term and resulted in divergent series.

In the third of the five examples of the Stieltjes thesis, a semi-convergent series expansion for $\log \Gamma(ai)$ was developed. As explained in the introductory section of his thesis, Stieltjes was interested in providing information of the behaviour of the function $\dfrac{1}{\Gamma(z)}$ where the variable z was purely imaginary. Stieltjes said:

> "Nous arrivons maintenant à un exemple tiré de la théorie de la fonction Γ. Aprés avoir rappelé en quelques mots le résultat principal des nombreuses recherches auxquelles a donné lieu l'étude de la série qui sert à calculer $\log \Gamma(a)$, nous considérons une autre série, n'ayant rien à ajouter à un sujet qui est si bien exposé dans la première Partie du travail de M. Bourguet sur les intégrales eulériennes. La considération de $\log \Gamma(ai)$ conduit à une série de seconde espèce, composée des mêmes termes que la série de Stirling dont nous faisons l'étude. Le résultat auquel nous arrivons permet de se faire une idée nette de la manière dont se comporte la fonction holomorphe $\frac{1}{\Gamma(z)}$, lorsque la variable z décrit l'axe des y."[6] (Stieltjes & van Dijk (ed.) 1993a, p.206)

The goal of the third example was to find an expansion of $\log \Gamma(ai)$ with a clear expression for the remainder term. In the introduction, Stieltjes mentioned the Stirling series but did not emphasize it in the same way that both Stokes and Poincaré did. They both pointed to the Stirling asymptotic approximation in a manner that was intended to reassure — a comparison with something similar that most mathematicians were aware of and had made use of. Stieltjes did not do this.

Stieltjes summarized the Stirling semi-convergent series by claiming that the first rigorous work on it had been done by Cauchy who, Stieltjes claimed, started from a formula that Jacques Philippe Marie Binet (1786-1856) found. It was in a paper of 1839 that Binet gave two integral representations for

$$\mu(x) = \ln \Gamma(x) - (x - \frac{1}{2}) \ln x + x - \frac{1}{2} \log 2\pi$$

which are now called Binet's formulas (Roy 2011, p.480). Cauchy used these formulas to prove, in 1843, that the error in using the formula for $\ln \Gamma(x+1)$ was of the order of the first term omitted (Roy 2011, p.479).

[6] We now come to an example taken from the theory of the Γ function. After recalling in a few words the main result of the numerous researches to which the study of the series used to calculate $\log \Gamma(a)$ has given rise, we consider another series, having nothing to add to a subject which is so well exposed in the first part of the work of Mr. Bourguet on Eulerian integrals. The consideration of $\log \Gamma(ai)$ leads to a series of the second kind, composed of the same terms as the Stirling series which we are studying. The result we arrive at allows us to get a clear idea of how the holomorphic function $\dfrac{1}{\Gamma(z)}$ behaves, when the variable z describes the y axis.

The semi-convergent expansion for $\log\Gamma(z)$ is typically written as follows:

$$\begin{aligned}\ln\Gamma(z) &= \frac{1}{2}\ln(2\pi) + \left(z - \frac{1}{2}\right)\ln z - z + \sum_{n=1}^{\infty} \frac{B_{2n}}{2n(2n-1)z^{2n-1}} \\ &= \frac{1}{2}\ln(2\pi) + \left(z - \frac{1}{2}\right)\ln z - z + \frac{1}{12z} - \frac{1}{360z^3} + \frac{1}{1260z^5} - \dots\end{aligned}$$

Stieltjes started with a formula due to Binet

$$\log\Gamma(a) = \left(a - \frac{1}{2}\right)\log a - a + \frac{1}{2}\log 2\pi + \int_0^\infty \left(\frac{1}{e^u - 1} - \frac{1}{u} + \frac{1}{2}\right)\frac{e^{-au}}{u}du$$

which he rewrote as

$$\log\Gamma(a) = \left(a - \frac{1}{2}\right)\log a - a + \frac{1}{2}\log 2\pi + \frac{1}{\pi}\int_0^\infty \frac{du}{1+u^2}\log\left(\frac{1}{1 - e^{-2\pi a u}}\right)$$

because it was easier to find the expression for the remainder term from this form of the function. The variable a is real.

Stieltjes claimed Louis Bourguet (1678-1742) had found the index of the smallest term of the expansion to be

$$\pi a + \frac{3}{4} - \frac{3}{32\pi a}$$

Stieltjes used his method of setting $\frac{dR_n}{dn}$ to zero to find the value of n that gave the smallest error bound since the remainder term he found was a semi-convergent series of the first kind. This gave the following value for n:

$$n = \pi a + \frac{1}{4} - \frac{13}{96\pi a}$$

It appears that Stieltjes' purpose with this analysis was simply to show that he could find the index of the minimum term more accurately.

After this Stieltjes considered the semi-convergent expansion of $\log\Gamma(ai)$ — that is, for arguments of the Γ function that were purely imaginary. He started with what he claimed was the definition of the Γ function adopted by Gauss:

$$\Gamma(ai) = \lim_{n\to\infty} \frac{e^{ai\log(n)}}{ai(1+ai)\left(1+\frac{ai}{2}\right)\cdots\left(1+\frac{ai}{n}\right)}$$

potentially to avoid the problem of taking logarithms of imaginary quantities. In this analysis, stated later, a was constrained to being positive.

After several pages of analysis taken from this starting point, Stieltjes concluded that it was possible to change a to ai in Binet's formula without introducing error. As we saw earlier, this was mostly a replication of the work that Stieltjes had sent to Hermite in the letter of February 13, 1886.

6.2.5 Finding Semi-convergent Expansions Involving a Differential Equation

The fourth example in the thesis involved finding semi-convergent series for integrals or divergent series which were solutions to a given differential equation. This is the closest example to those of both Stokes and Poincaré whose main motivation for asymptotic expansions was the numerical approximation of functions which were solutions of differential equations with important application in physics.

Only in the introduction to his work did Stieltjes say anything about applications. He stated that the differential equation of interest

$$\frac{d^2z}{da^2} + \frac{1}{a}\frac{dz}{da} + z = 0$$

arose in several questions from mathematical physics. This is the Bessel differential equation of order zero with solution

$$c_1\mathrm{J}_0(a) + c_2\mathrm{Y}_0(a).$$

This was the same differential equation that Stokes used during his effort to find values of the integral

$$u = \frac{2}{\pi}\int_0^{\frac{\pi}{2}} \cos(x\cos\theta)d\theta$$

Unlike Stieltjes, Stokes did not start with the differential equation. Stokes started with an integral for which he wanted to compute values for large values of the parameter x. He then determined that the integral was a solution of the Bessel differential equation and then he used assumptions about the important terms in the differential equation to develop his asymptotic expansion. For Stieltjes, the only use of the differential equation was to check that after manipulating the integral solutions of the differential equation into an asymptotic expansion, they remained solutions of the differential equation.

The evaluation of these integrals using contour integration for the function $\mathrm{J}(ai)$ followed over many pages of analysis. The steps that were taken were justified by various means including: substitution into the differential equation, comparison with the results of Weber, and comparison with results of Riemann. This allowed Stieltjes to state that

$$\mathrm{J}(ai) = \frac{e^a}{\pi^2}\int_0^\infty\int_0^\infty \frac{e^{-2au}}{1-u+v}\frac{du\ dv}{\sqrt{uv}}$$

where the principal value of the integral was to be used.

Stieltjes then used the technique that he used in all previous examples to turn this integral into a semi-convergent series — that is, he used a geometric series identity. In this case it was the following identity:

$$\frac{1}{1-u+v} = \frac{1}{1+v} + \frac{u}{(1+v)^2} + \cdots + \frac{u^{n-1}}{(1+v)^n} + \frac{u^n}{(1+v)^n(1-u+v)}$$

which he found by dividing $(1+v)-u$ into 1 considering u as the variable.

This allowed Stieltjes to find the semi-convergent expression for $\mathrm{J}(ai)$ along with, critically, an expression for the remainder term for which he then solved the transcendent equation $R_n = 0$.

$$\mathrm{J}(ai) = e^a\sqrt{\frac{1}{2a\pi}}\left[1 + \frac{1^2}{1\cdot 8a} + \frac{1^2\cdot 3^2}{1\cdot 2\cdot (8a)^2} + \cdots + \frac{1^2\cdot 3^2\ldots(2n-3)^2}{1\cdot 2\ldots(n-1)(8a)^{n-1}}\right] + R_n$$

where

$$R_n = \sqrt{\frac{2a}{\pi^3}}\int_0^\infty\int_0^\infty \frac{u^n e^{-2au}}{(1+v)^n(1-u+v)}\frac{dudv}{\sqrt{uv}}$$

After several pages of analysis of the integral for R_n, Stieltjes found the following series representation for R_n:

$$R_n = e^{-2a}\sqrt{\frac{4a}{\pi n^2}}\left[\eta + \frac{2}{3} + \left(\frac{1}{6}\eta^3 - \frac{1}{2}\eta^2 - \frac{1}{2}\eta - \frac{179}{540}\right)\frac{1}{n} + \cdots\right]$$

from which the value of n such that $R_n = 0$ was approximated. That value of n was used to determine the number of terms to use from the semi-convergent series of $\mathrm{J}(ai)$.

In the conclusion of the section pertaining to this example, Stieltjes compared his result to that of Riemann and concluded that he had confirmed and clarified Riemann's result. Stieltjes said:

"Riemann, en donnant la série semi-convergente, écrivait

$$\mathrm{J}(ai) = e^a \sqrt{\frac{1}{2\pi a}} \sum_{n<2a+1} \frac{[1 \cdot 3 \cdots (2n-1)]^2}{1 \cdot 2 \cdots n (8a)^n}$$

et il ajoutait qu'on ne peut calculer ainsi $\mathrm{J}(ai)$ qu'en négligeant des parties de l'ordre e^{-2a} vis-à-vis de l'unité. Le résultat auquel nous sommes arrivé confirme et précise ces indications."[7] (Stieltjes & van Dijk (ed.) 1993a, p.252)

This example and, similarly, the final example of the thesis regarding the function $P(a) = \sum_1^{\infty} \frac{1}{e^{\frac{n}{a}} - 1}$ considered at the beginning of this section, appear to have been chosen to show that Stieltjes could, by his new method, produce a semi-convergent series that agreed with what had been previously obtained. And further, Stieltjes showed that his introduction of an additional semi-convergent series to determine when the reminder term changed sign gave a value of n that, when used to compute a value for the functions under consideration, gave a better bound on the error on the approximation than had been previously stated.

6.3 Concluding Remarks on Stieltjes and Semi-convergent Series

Throughout the thesis, Stieltjes referred to several different mathematicians and their work — Schlömilch, Bourguet, Poisson, Lipschitz, Riemann, Laplace, and Weber. I have listed all of these people because I claim that, despite the fact that Stieltjes' thesis was published in exactly the same year as Poincaré's paper on asymptotic expansions, his work was done independently of Poincaré. This is also the conclusion of Kline (Kline 1990, p.1104).

There was no mention of Poincaré in Stieltjes' thesis and, unsurprisingly, there was also no mention of the work of Stokes from 36 years earlier. In Poincaré's work of 1886 on asymptotic expansions there was, reciprocally, no mention of Stieltjes; however it is unlikely that Poincaré was unaware of Stieltjes since there was a common contact through Hermite. Certainly by 1894, Poincaré was well aware of Stieltjes' work on continued fractions. When

[7] Riemann, giving the semi-convergent series, wrote

$$\mathrm{J}(ai) = e^a \sqrt{\frac{1}{2\pi a}} \sum_{n<2a+1} \frac{[1 \cdot 3 \cdots (2n-1)]^2}{1 \cdot 2 \cdots n (8a)^n}$$

and he added that one can thus calculate $\mathrm{J}(ai)$ only by neglecting parts of the order e^{-2a} with respect to unity. The result we arrived at confirms and clarifies these indications.

that work was awarded a prize by the *Académie des Sciences*, Poincaré said, on behalf of the jury:

> "Le travail de M. Stieltjes est donc un des plus remarquables Mémoires d'analyse qui aient été écrits dans ces dernières annéees; il s'ajoute à beaucoup d'autres qui ont placé leur auteur à un rang éminant dans la Science de notre époque.... La commission a l'honneur de proposer à l'Académie d'accorder à, M. Stieltjes le plus haut témoignage de son approbation en ordonnant l'insertion de son Mémoire "Sur les fractions continues" dans le Recueil des Savants étrangers (à l'Académie) et elle émet le vœu qu'un prix puisse lui être accordé sur la fondation Lecomte"[8] (Stieltjes & van Dijk (ed.) 1993a, p.3)

The work of Stieltjes on asymptotic expansions had points in common with both the work of Stokes and Poincaré but also exhibited noticeable differences. In comparing the work of Stieltjes with Stokes, one similarity was the comparison of the numbers obtained from the asymptotic expansions to exact results or observations. That these comparisons were favourable was then a justification for the use of divergent series. The final statement of Stieltjes' thesis was a numerical example designed to show how good the approximations he obtained were. This is reminiscent of how Stokes decided his approximations were good.

Stieltjes' method was completely different from the one Stokes used to obtain an asymptotic approximation — one that had the distinct advantage of providing an error bound for the approximation. Further, unlike Stokes, there was no recourse to any differential equation to which the function or integral to be approximated may be a solution. In contrast, the method of Stokes relied completely on assumptions about the differential equation to which the function to be approximated was a solution.

Like both Stokes and Poincaré, Stieltjes similarly compared the asymptotic approximations to results obtained by other means. Again and significantly, the Stieltjes method produced an error bound — something that was not present in the work of Stokes. Stieltjes' major advance over Stokes was that he had a limited method that worked without the need for an attendant differential equation. His method also produced an error bound.

[8]Therefore Stieltjes' work is one of the most remarkable memoirs in analysis which have been written in the past years; it adds to many others which have placed their author in an eminent rank within the Science of our period... The committee takes pride in proposing the Academy award Mr. Stieltjes the highest evidence of his approval by ordering the insertion of his memoir "Sur les fractions continues" into the Recueil des Savants étrangers (à l'Académie) and the committee expresses the wish that a prize could be awarded him from the Lecomte foundation.

Copson (Copson 1965), whose work on asymptotic expansion is elaborated on in the conclusion to this thesis, said that Stieltjes' ability to produce an error bound was a notable feature of his thesis. Copson said:

> "During the nineteenth century, asymptotic expansions were obtained for many of the special functions of analysis, sometimes only formally, sometimes with a rigorous discussion of the order of magnitude of the error. Of particular interest is Stieltjes's doctorate thesis in which he examined the error committed by stopping at the least term of the asymptotic representations of certain important special functions, and showed how the approximation so obtained could be improved." (Copson 1965, p.3)

Stieltjes and Poincaré did their work in the same year and, as noted earlier and worth repeating, Stieltjes' statement about what a semi-convergent expansion was a less sophisticated statement than that of Poincaré who defined S_n to be the asymptotic representation of the function $\mathrm{J}(x)$ if the expression

$$x^n\left(\mathrm{J} - S_n\right)$$

tended to zero when x increased indefinitely.

Stieltjes simply wrote down the general form of a divergent series and then claimed that for large values of the variable the coefficients in the series could sometimes be found by taking limits. As we shall see, it is the Poincaré definition that became the standard definition.

When Stieltjes found the semi-convergent expansion for $\dfrac{\sin(u)}{1+u^2}$ he did so from his first principles. This was despite that fact that he had already found the asymptotic expansion for the logarithmic integral and he could have used that algebraically to find the asymptotic expansion for $\dfrac{\sin(u)}{1+u^2}$. Poincaré, on the other hand used several pages of his paper to establish and prove the mathematical operations permitted with asymptotic expansions before finding any expansions.

This opinion of the differences between the Stieltjes and Poincaré work on asymptotic expansion is not fully shared by Schlissel, who claimed that the definitions of Poincaré and Stieltjes are the same, but who agrees that Stieltjes did not develop the theorems that Poincaré did. Further Schlissel claimed that Stieltjes found the asymptotic expansion of the Bessel equation by a highly specialized procedure — a statement with which I agree because Stieltjes' procedure only works for certain types of functions. Schlissel said:

> “The same definition of asymptotic series was given in the doctoral dissertation of the Swedish [sic] mathematician Stieltjes [1886] without, however, any of the theorems developed by Poincaré. Stieltjes found the asymptotic series solution for the Bessel equation of order zero by a highly specialized procedure.” (Schlissel 1977, p.327)

For the reasons enumerated above, I claim that there was a higher level of rigour and sophistication in Poincaré’s work compared to the more ad hoc or heuristic methods of Stokes or Stieltjes. This is a likely reason why, in later work, the vocabulary of Poincaré and his methods were used to build the fully rigorous theory of asymptotic expansions. In the conclusion of this thesis, I comment further on the reception of the work of Stokes, Poincaré, and Stieltjes in the years following 1886.

Chapter 7

Conclusion

In this thesis, I have provided detailed analysis of the 1856 work of Stokes, the 1886 work of Poincaré, and the 1886 work of Stieltjes, all of which involve divergent series and the development of asymptotic expansions. I have also examined additional work of these three individuals and the work of other contemporaneous mathematicians to provide mathematical context. Further, I have analyzed the social context — in particular the role of precision — of the mathematical work studied in this thesis and I have demonstrated the difference between the British and the French approach to mathematics during this time and for this subject.

I have shown that, independently, and for differing reasons, these three people used asymptotic expansions in their computations. Stokes, Poincaré and Stieltjes had different motivations for using asymptotic expansions and each of their approaches was different. Further, their work had differing levels of impact on the fuller development of asymptotic expansions that took place in the first half of the twentieth century.

The early reception of the work of these three mathematicians provides a method of assessing the immediate impact of their work. A look at the mature theory of asymptotic expansions fifty years on provides a picture of how the work analyzed in this thesis became a standard tool of analysis. I will use both methods in the discussion that follows.

The textbooks and papers of Whittaker are used to assess the early reception of the theory of asymptotic expansions done by Stokes, Poincaré and Stieltjes. The work of Copson and Erdélyi provide a picture of how this early work on asymptotic expansions described in this thesis became a standard part of numerical analysis which arose as a distinct field of study in the early twentieth century (Maidment 2021).

7.1 Whittaker's Early Response to Poincaré's Work

Edmund Taylor Whittaker graduated as Second Wrangler in the mathematical tripos in 1885 and a year later was elected to a fellowship at Trinity College, Cambridge. In 1905, he was elected a fellow of the Royal Society and, among many other awards, he was knighted in 1945. He spent the bulk of his career as a professor of mathematics at the University of Edinburgh. His main interests were in fundamental mathematical physics and, along with many important mathematical papers, he authored several textbooks which have become classics. For basic biographic and scientific information on Whittaker, see (Martin 2008). For a more fulsome account of Whittaker's life, both as an academic and as a person, and for an analysis of the large impact he had on the development of the field of numerical analysis through the founding of Britain's first mathematical laboratory, see (Maidment 2021) and (Maidment & McCartney 2019).

In September of 1899, the sixty-ninth meeting of the *British Association for the Advancement of Science* was held in Dover. The report of this meeting was published in 1900 and, in that report, was a lengthly article written by Whittaker titled *Report on the Progress of the Solution of the Problem of Three Bodies* (Whittaker 1900). The was the fulfilment of Whittaker's obligation to provide, to the BAAS, a comprehensive summary of the development of planetary theory between 1868 and 1898. Whittaker worked for two years (Maidment 2021, p.46) on this report with the consequent result that he was then recognized as an authority on celestial mechanics. For example, in 1915, the previous Royal Astronomer of Ireland, Robert Stawell Ball (1840-1913) said:

> "Then as to his scientific attainments, he knows more of the mathematical part of astronomy than anyone else in Great Britain, or if you like to add Europe, Asia, Africa, and America, I won't demur." (Maidment & McCartney 2019, p.185)

In the report, Whittaker explained that the fundamental problem of dynamical astronomy was a determination of the orbits of mutually attracting particles according to Newtonian law and that the solution to this problem relied on the solution of differential equations via infinite series. Whittaker said:

> "The solution of the problem depends on the integration of a system of differential equations; and various methods have been given for the solution of the equations by means of infinite series of known functions. The methods are, however, in general cumbrous; the convergence of the series employed has only

recently been considered with any success, and the true nature of the integrals of the problem is unknown." (Whittaker 1900, p.121)

Whittaker used the phrase "the true nature of the integrals" where the integrals, as he said, were solutions to differential equations that were found in the form of infinite series. The true nature of the solutions was the behaviour of the solutions over a long time interval. This was difficult to determine either because of slow convergence or because of the divergence of the infinite series solutions. The question of the stability of the solar system required answers to questions about exactly this type of behaviour and this was unknown.

Whittaker chose to limit the scope of his report to the theory that considered solely the mathematical discussion of the fundamental problem as he stated it. This meant excluding material that pertained to numerical applications or to the suitability of developments for the purposes of computation. Whittaker reported on developments up to and including the last volume of Poincaré's *Les méthodes nouvelles de la mécanique céleste*. He therefore read Poincaré's entry to the prize competition on the three body problem and saw asymptotic expansions being used there. Thus, he also saw Poincaré's reference to his 1886 paper on the theory of asymptotic expansions.

According to Whittaker, the modern era of celestial mechanics began with Hill who changed the way the three body problem was approached. Rather than starting with the solution to the two body problem and perturbing that solution to account for the introduction of a third body, Hill started by solving the restricted three body problem directly.

Hill was an American astronomer and mathematician who, as we saw earlier, responded quickly and rather negatively to Poincaré's statements about the divergence of infinite series in celestial mechanics. Simon Newcomb (1835-1909), director of the *American Ephemeris* project called Hill "the greatest master of mathematical astronomy during the last quarter of the nineteenth century" (Eisele 2008, p.398). As part of his work for this project, Hill reconstructed the theories and tables of lunar and planetary motion. These calculations were a particular case of the three body problem (Eisele 2008, p.399).

According to Whittaker, and he likely meant prior to 1886, Hill did not consider as least some types of convergence issues. For example, Hill computed the motion of the lunar perigee, by solving — what Whittaker called — Hill's equation or the generalized Gyldén-Lindstedt equation which resulted in infinite series in the form

$$w = \sum_{r=-\infty}^{\infty} a_r \cos\{(c + 2r)t + a\}.$$

The method Hill used transformed this series into an infinite number of homogeneous linear equations. The value of c which gave the determinant of infinite coefficient matrix of this linear system a value of zero was used to determine the motion of the lunar perigee. As we saw earlier, Hill wrote about the convergence of infinite series in 1896. Whittaker said:

> "The convergence of the infinite determinant was not considered by Hill; this gap in the work was filled by Poincaré in 1886." (Whittaker 1900, p.132)

By 1890, Whittaker claimed that Poincaré had made progress on the question of stability of the solar system by demonstrating the existence of asymptotic solutions to the restricted three body problem which implied an infinite number of solutions stable in the Poisson sense. An asymptotic solution, as per Whittaker, was a collection of the asymptotic expansions of all of the functions that satisfied the differential equations of celestial mechanics for the three body problem. Whittaker also used the term asymptotic solution for what Poincaré called an asymptotic expansion and when quoting or discussing Whittaker, I have used his term asymptotic solution.

The restricted three body problem, as discussed in Chapter 4, is the three body problem where one of the three bodies has a negligible mass relative to the other two and thus exerts no influence on the motion of the other two bodies. The zero mass body is assumed to orbit around the centre of mass of the other two bodies and in the plane of orbit of those two bodies. Poincaré considered two types of stability. Poisson stability meant that an orbiting body returns to within epsilon of its initial position an infinite number of times. The other type of stability requires an orbiting body to stay within epsilon of its previous orbits.

Whittaker said:

> "The existence of asymptotic solutions (which will be explained later) shows that an infinite number of particular solutions of the restricted problem of three bodies exist, which are not stable in Poisson's sense of the word. But M. Poincaré now proves that there are also an infinite number which are stable, and, further, that the former are the exception and the latter are the rule, in the same sense as commensurable numbers are the exception and incommensurable numbers are the rule. In other words, the probability that the initial circumstances may be such as to give rise to an unstable solution is zero." (Whittaker 1900, p.145)

The probabilistic comment about the likelihood of unstable solution was phrased in a surprising manner given that Whittaker's report pre-dates measure theory. Whittaker commented in general on the two types of stability, linked them together and said that one type of stability implied the other. Whittaker said:

> "Poincaré's memoirs of 1881-6 on curves defined by differential equations lead to one result of importance in Dynamical Astronomy. In order that the system of n bodies may be stable, two conditions must be fulfilled: firstly, the mutual distances must always remain within certain limits; and, secondly, if the system has a definite configuration at any instant, it must be possible to find a subsequent instant at which the configuration differs from this as little as we please. It follows from the investigations of this series of memoirs that, if the first of these conditions is satisfied, the second is also." (Whittaker 1900, p.133)

I focus next on what Whittaker said about asymptotic solutions. Whittaker's first explanation of an asymptotic solution, in the 1899 report, was that it was a solution that approached the original periodic solution more and more closely as the time increases. Whittaker claimed that Poincaré developed the theory of asymptotic solutions for the differential equations of dynamics.

Shortly after the above claims, Whittaker stated that the asymptotic solutions used in celestial mechanics are not convergent and belong to an "important class of developments which are now called *Asymptotic Expansions* [his emphasis]" (Whittaker 1900, p.149). Whittaker claimed that the best known examples of this kind series were: the Stirling series for the Γ-function, and the "so-called semiconvergent expansions" for the Bessel functions and the Riemann ζ-function.

Whittaker then summarized Poincaré's example from the 1886 paper,

$$F(w, \mu) = \sum_n \frac{w^n}{1 + n\mu} = \sum_{n,p} w^n (-n)^p \mu^p,$$

detailed in Chapter 5 of this thesis, and provided the Poincaré definition of what it meant for an infinite sum to be an asymptotic series representation of the original function. Whittaker followed that with a summary of Poincaré's results in celestial mechanics from *Sur le problème des trois corps et les équations de la dynamique* (Poincaré & Popp 2017).

Subsequent to the work in 1886 and 1890, Poincaré's *Les Méthodes Nouvelles de la Mécanique Céleste* was published in three volumes in 1892, 1893 and 1898. Of this work and relating to asymptotic expansions Whittaker reported the following:

> "The theories of periodic solutions, characteristic exponents, asymptotic solutions, and the non-existence of uniform integrals were somewhat more completely discussed in 1892 by Poincaré himself in the first volume of his treatise on the new developments of dynamical astronomy. The second volume, which

was published in 1893, and contains a good deal of matter which had not appeared in the memoir of 1891, opens with a chapter on asymptotic expansions" (Whittaker 1900, p.152)

The preceding analysis of Whittaker's 1899 British Association report shows that Whittaker learned of asymptotic expansions from Poincaré while reviewing Poincaré's work on celestial mechanics. It was only shortly after this report that Whittaker authored *A Course in Modern Analysis* (Whittaker 1902) published in 1902. This text was revised and a second author, George Neville Watson (1886-1965), was added resulting in a second edition published in 1915. *A Course in Modern Analysis* was a standard analysis textbook for many years in England and is still in print with a fifth edition printed in 2021.

That this textbook was influential and important can be shown in several ways. For example, Hardy in his 1947 text *Divergent Series* referred to his former teacher Whittaker's text for the proof of Stirling's theorem. Further, in 1941, *A Course in Modern Analysis* was included among a selected list of mathematical analysis books for use in universities published by *American Mathematical Monthly* (Moulton 1941). In the view of June Barrow-Green (Barrow-Green 2002), Whittaker and Watson's *Modern Analysis* is "one of the most enduring of the English mathematics textbooks of the twentieth century" and was for many years "virtually the only book in English to give an introductory account of the methods of analysis, and of the special functions used in mathematical physics".

Stokes, as seen in Chapter 3, did not give a definition of an asymptotic expansion. Both Stieltjes and Poincaré provided definitions. Whittaker gave essentially the Poincaré definition in his textbook and he used the name asymptotic expansion as coined by Poincaré. The Stieltjes term semi-convergent was used in the Whittaker text but it was used for what is now typically called a conditionally convergent series.

Chapter 8 of the 1902 Whittaker textbook was titled *Asymptotic Expansions*. This textbook was published just sixteen yeas after the work of Poincaré and Stieltjes and just three years after Whittaker's British Association Report on celestial mechanics. It therefore serves to show how the three introductions of asymptotic expansions into the mathematical literature analyzed in this thesis were disseminated, at least in English.

Whittaker began Chapter 8 with a divergent series representation for the convergent integral $f(x) = \int_x^\infty \frac{e^{x-t}\,dt}{t}$ produced by repeatedly integrating by parts. The infinite series obtained for $f(x)$ diverged for all values of x. Whittaker bounded the error in using this divergent series to approximate the function for large values of x by approximating the remainder integral with an inequality that required that $x > 2n$ where n was the number of terms of the asymptotic expansion used in the approximation.

This motivating example was followed by the Poincaré definition of an asymptotic expansion. Whittaker said:

> "A divergent series
>
> $$A_0 + \frac{A_1}{x} + \frac{A_2}{x^2} + \cdots + + \frac{A_n}{x^n} + \cdots$$
>
> in which the sum of the first $(n+1)$ terms is S_n is said to be an *asymptotic expansion* of a function $f(x)$, if the expression $x^n\{f(x) - S_n\}$ tends to zero as x (supposed for the present to be real and positive) increases indefinitely." (Whittaker 1902, p.164)

Whittaker tells us that this definition was Poincaré's:

> "The definition which has just been given is due to Poincaré. Special asymptotic expansions had, however, been discovered and used in the eighteenth century by Stirling, Maclaurin and Euler. Asymptotic expansions are of great importance in the theory of Linear Differential Equations, and in Dynamical Astronomy; these applications are, however, outside the scope of the present work, and for them reference may be made to Schlesinger's *Handbuch der Theorie der linearen Differentialgleichungen*, and the second volume of Poincaré's *Les Méthodes Nouvelles de la Mécanique Céleste*." (Whittaker 1902, p.165)

Ludwig Schlesinger (1864-1933) was a German-educated mathematician whose handbook on linear differential equations was published in two volumes in 1895 and 1897. Whittaker referred directly in the text quoted above to Poincaré's *Les Méthodes Nouvelles de la Mécanique Céleste*, and he footnoted, in the above quote, the definition given in Poincaré's 1886 paper.

Whittaker had, so far, found an asymptotic expansion for an integral using integration by parts where the analysis was for real and positive values of the variable. He also gave the Poincaré definition of an asymptotic expansion. He followed the definition with another example where he used the geometric series to take a convergent infinite series with terms $\frac{c_k}{x+k}$ and produce a divergent series — a procedure which Whittaker said was "not legitimate" because the resulting series was divergent. However, he showed the resulting series was an asymptotic expansion. Whittaker said:

> "If, therefore, it were allowable to expand each fraction $\frac{1}{x+k}$ in this way, and to rearrange the series according to descending powers of x, we should obtain

the series

$$\frac{A_1}{x} + \frac{A_2}{x^3} + \ldots + \frac{A_n}{x^n} + \ldots$$

where

$$A_1 = \sum_{k=1}^{\infty} c^k; \quad A_2 = -\sum_{k=1}^{\infty} kc^k, \text{ etc.}$$

But this procedure is not legitimate, and in fact the series diverges. We can, however, shew that the series is an asymptotic expansion of $f(x)$, which will enable us to calculate $f(x)$ for large values of x." (Whittaker 1902, p.166)

Using the geometric series outside of its interval of convergence was the main method of finding asymptotic expansions in the 1886 work of Stieltjes. There was no mention of Stieltjes in the Whittaker text.

Whittaker then continued, as in Poincaré, to show that two asymptotic expansions can be multiplied and that an asymptotic expansion can be integrated term by term. He further showed that a given series can be an asymptotic expansion of several distinct functions but that a function cannot be represented by more than one distinct asymptotic expansion for real positive values of the variable. The careful statement of the domain indicates that Whittaker was likely aware of the Stokes line phenomenon. Chapter 8 of the Whittaker text on asymptotic expansions concluded with exercises where exercise 4 was the Schlömilch example as seen in the Stieltjes thesis.

The differences between the first (1902) and the second (1915) editions of *Modern Analysis* provide insight about how the theory of asymptotic expansions was maturing in the first part of the twentieth century. There were three main differences between the 1902 and the 1915 text: numeric justification was added, the symbol $\sim$ was introduced to mean asymptotically equal, and there was an additional section on summability theory added to the chapter which likely came from the Bromwich text as we shall describe below.

Numeric justification was added to the first example ($f(x) = \int_x^{\infty} \frac{e^{x-t}\,dt}{t}$) of the Chapter 8. Five terms of the asymptotic expansion were used to compute an approximation for $f(10)$. These choices for n and x maintained the necessary relationship between x and n that allowed an error bound to be computed. Whittaker found that

"Taking even fairly small values of x and n

$$S_5(10) = .09152, \text{ and } 0 < f(10) - S_5(10) < .00012."$$

(Whittaker & Watson 1915, p.151)

The title of Chapter 8 was changed to *Asymptotic expansions and Summable Series* in the 1915 edition. The new material added to Chapter 8 was on methods of summing series. This is another way of considering divergent series where the methods of summability theory are used to assign a number to a divergent series. Note that summability theory developed later than asymptotic analysis and this is reflected in the differences between the first and second editions of the Whittaker and Watson text.

Finally, unlike the first edition of *A Course in Modern Analysis*, the second edition included a list of references at the end of the chapter on asymptotic expansions. Those references were to work of Poincaré (first) followed by Borel, Bromwich, Barnes, Hardy/Littlewood, Watson, and Chapman. There was no mention of Stokes or of Stieltjes.

The reference to Thomas John I'Anson Bromwich (1875-1929) deserves particular attention because Bromwich wrote *An Introduction to the Theory of Infinite Series* (Bromwich 1908), published in 1908, midway between the first and second editions of Whittaker's text. In the preface of the work, Bromwich explained that his textbook was based on lectures in elementary analysis he gave at Queen's College, Galway between 1902 and 1907 (Bromwich 1908, p.v). However, Chapter XI, titled *Non-convergent and Asymptotic Series* was added when preparing the manuscript for publication.

The first paragraph of Chapter XI, which Bromwich claimed "contains a tolerably complete account of the recently developed theories of non-convergent and asymptotic series" (Bromwich 1908, p.ix), listed the principal sources from which Bromwich derived the material for the chapter. In full, this list consisted of three papers of Borel (1901, 1896, 1899), one paper of Cesàro (1890), two papers of Hardy (1904, 1903), a paper of E. Le Roy (1902), the 1886 paper of Poincaré analyzed in this thesis, a paper by Van Vleck (1905), and a paper by Vivanti (1906). Bromwich continued the chapter with a historical summary followed by a presentation of both summability theory and asymptotic expansions.

Bromwich presented a fairly standard history of asymptotic expansions where he began with the older analysts (pre Abel and Cauchy) who had "little hesitation in using non-convergent series both in theoretical and numerical investigations"(Bromwich 1908, p.261). These early analysts, claimed Bromwich, used only series now called asymptotic. He further claimed that the sums were used to the point where the terms were sufficiently small. According to Bromwich, an important type of asymptotic series were later used in astronomy to calculate planetary motion and it was Poincaré who showed that these series did not converge and who explained why, even though the results of calculations using asymptotic expansions were confirmed by observation. Bromwich said:

> “An important class of such series consists of the series used by astronomers to calculate the planetary positions: it has been proved by Poincaré that these series do not converge, but yet the results of the calculations are confirmed by observations. The explanation of this fact may be inferred from Poincaré’s theory of asymptotic series.” (Bromwich 1908, p.262)

Bromwich then noted Cauchy’s work which established the asymptotic property of the Stirling series and claimed that the possibility of obtaining other useful asymptotic series was generally overlooked following Cauchy, with the exception of Stokes, who, Bromwich stated, published three remarkable papers in 1850, 1857 and 1868. That work of Stokes is extensively discussed in Chapter 3 of this thesis. Then, according to Bromwich, came the 1886 papers of Stieltjes and Poincaré, discussed in Chapters 5 and 6 of this thesis. This is the only time where I have seen the work of Stokes referred to in a synopsis of the development of asymptotic expansions. Further, Whittaker and Watson (1915) references this Bromwich text but the book does include the reference to the Stokes papers.

Chapter 11 of the Bromwich text is mostly about summability theory. The chapter is divided into 43 articles, of which six articles concern asymptotic expansions. Articles 130 and 131 consist of Euler’s method of using an alternating series asymptotic expansion to find a value for the constant now usually referred to as Euler’s γ constant. Thus Bromwich bounded the error to the first term omitted. He clearly pointed out that, unlike the error bounds for approximations using convergent series, it was not possible to push the approximation to an arbitrary degree of accuracy when using an asymptotic expansion. Article 132 consists of three expansions and their derivation — the logarithmic integral, the Fresnel integrals, and the Stirling series.

Of the remaining three articles on asymptotic expansions, two of them consist of a summary of the work of the Poincaré along with a discussion of the application of the work of Poincaré to the solution of differential equations. The final article gives a simple method attributed to Stokes for dealing with certain types of real series.

I conclude that it was the work of Poincaré that most influenced the development, in Britain, of the theory of asymptotic expansions. This is primarily based on the mathematical content of Chapter 8 of first edition of *A Course in Modern Analysis*, the statements made in that chapter about where the definitions originated, and the references at the chapter end of the second edition.

Whittaker’s 1899 summary of progress on the three body problem and his 1902 textbook are part of a bigger picture of the genesis of the field of numerical analysis at the turn of the twentieth century. It is difficult to determine exactly when numerical analysis became a field of study in its own right. As we have seen, in part, in this thesis, there are many and varied methods of obtaining numbers from theory. Maidment, for example, in a paper titled, *The Edinburgh Mathematical Laboratory and Edmund Taylor Whittaker's role in the early development of numerical analysis in Britain* (Maidment 2021), said the following:

> "It is difficult to pinpoint when numerical analysis became a field in its own right. Various techniques that now form the subject had been worked on since antiquity. Dominique Tournés explains that the techniques developed in astronomy, celestial mechanics, and rational mechanics led to the first professional applied mathematicians in the late 19th and early 20th centuries, which was approximately the time when the numerical analysis we know today became an autonomous discipline." (Maidment 2021, p.41)

Whittaker's role in the genesis of numerical analysis, with which asymptotic analysis is closely associated, is further revealed in his establishment of the first mathematical laboratory in Britain. Whittaker's decision to establish a mathematical laboratory, according to Maidment, was partially due to his interest in the practical techniques relating to astronomy (Maidment 2021, p.46). A second motivation for the laboratory, in a reflection of the discussion of precision from Chapter 2 of this thesis, was his friendship with a number of actuaries who were working in Edinburgh, a key centre for life assurance, and who were experiencing mathematical difficulties (Maidment 2021, p.46).

One important outcome of the laboratory was another influential textbook by Whittaker and co-author, George Robinson. Published in 1924, *The Calculus of Observations*, contained, among many other topics, techniques for the numerical solution of differential equations. Whittaker's work here was influenced by Carl Runge (1856-1927) who pioneered modern numerical analysis in Germany around the turn of the century.

Next, I briefly consider two later texts which were influential in England and the United States to show how asymptotic expansions became a standard tool of analysis.

7.2 Asymptotic Expansions at Mid-twentieth Century

I have chosen the 1956 text of Erdélyi and the 1965 text of Copson, both of which are titled *Asymptotic Expansions* because they are standard references on this topic and both authors make clear statements about their perception of the origin of the asymptotic expansions they describe. I have previously used quotes, in Chapter 5, from both Copson and Erdélyi to substantiate the importance of Poincaré in the initial development of asymptotic expansions.

Here, I provide more detail about Erdélyi and Copson's work partly to show how asymptotic expansions became a standard tool of analysis.

Erdélyi was born in Hungary in 1908 and educated in both Czechoslovakia and Germany. He was forced to flee Czechoslovakia in 1938 by the Nazis and, as a result, moved to the University of Edinburgh where he stayed until 1949 (Colton 2008, p.266). After his move to Edinburgh, Erdélyi was a consultant to the Admiralty Computing Service, set up 1943 to assist the admiralty with computing during wartime (Maidment 2021, p.58). He then moved to California Institute of Technology from which he returned to Edinburgh in 1964 where he stayed until his death in 1977 (Colton 2008, p.266).

Erdélyi's move to Caltech was precipitated by the death of Harry Bateman (1882-1946), a British mathematician who was a professor at Caltech, and who left behind a large volume of notes on special functions which Erdélyi was appointed to edit and publish. This Bateman project resulted in five volumes of reference handbooks titled *Higher Transcendental Functions* and *Tables of Integral Transforms* which became basic references for mathematicians and physicists worldwide until the need for them was removed by computing. At the conclusion of the Bateman project, Erdélyi studied asymptotic expansions of integrals and solutions of differential equations making several advances in the subject (Colton 2008, p.266).

Remarkably, this work of Erdélyi was influenced by work on boundary-layer theory then being done at Caltech and we see, for a second time, a relationship between asymptotic expansions and boundary-layer theory which we first saw with Stokes. David Colton, author of the *Complete Dictionary of Scientific Biography* article on Erdélyi summarized Erdélyi's work on asymptotic expansion in the following way:

> "Erdélyi demonstrated that the Poincaré-type definition of an asymptotic expansion is much too narrow for a satisfactory discussion of the asymptotic behavior of functions depending on more than one parameter. These investigations of asymptotic analysis were influenced by the work then being undertaken in the Guggenheim Aeronautical Laboratory at Caltech on the development of an improved boundary-layer theory for viscous fluid-flow past obstacles, and Erdélyi's lifelong interest in singular perturbation theory can be traced back to this time. His book *Asymptotic Expansions* appeared in 1956 and is now regarded as one of the classic monographs on the subject of asymptotic analysis." (Colton 2008, p.266)

Copson was born in England, educated at Oxford and immediately obtained a lectureship at Edinburgh upon graduation in 1922. He moved shortly after to the University of St.

Andrew's from which he retired in 1969 having spent time in the interim at Royal Naval College, University College, Dundee and Harvard University (Rankin 1981, p.564).

The work of both Copson and Erdélyi grew out of working papers and reports that were requested by their respective governments in support of military operations. Copson authored, in 1943, a short monograph titled *The Asymptotic Expansion of a Function Defined by a Definite Integral or Contour Integral* which was intended for use in Admiralty Research Establishments and was written at the request of the Director of the Admiralty Computing Services. This was one of a series on monographs on topics that were inadequately covered in easily accessible literature (Rankin 1981, p.565). This is perhaps indicative that in the years between Whittaker in 1902 or even 1915 and 1943 that there had not been much textbook treatment of the topic of asymptotic expansions. It also reflects the evolution of the importance of asymptotic expansions.

Copson's 1965 book was an enlargement that built upon work he had previously done at the request of the Admiralty Computing Service. In the preface to his 1965 book, Copson thanked Erdélyi for his generous advice and encouragement. Reciprocally, Erdélyi stated that the second chapter of his book, whose topic is the most important methods for the asymptotic expansion of functions defined by integrals, owed much to the excellent pamphlet on this subject by Copson. Both Copson and Erdélyi had a close personal connection to Whittaker. Whittaker helped Erdélyi flee from German-occupied Czechoslovakia and obtain a position at the University of Edinburgh and Copson was Whittaker's son-in-law.

In the case of Erdélyi, his 1956 book was an unabridged and unaltered replication of a technical report prepared under contract for the United States Office of Naval Research during the time that Erdélyi was at Caltech. Erdélyi footnoted the work of J.G. van der Corput from 1951 and 1952, again titled *Asymptotic Expansions*, which was contained in working papers prepared for the National Bureau of Standards.

Erdélyi's *Asymptotic Expansions* (Erdélyi 1956) was based on a course of lectures given in 1954 at Caltech whose purpose was to introduce students to "various methods for the asymptotic evaluation of integrals containing a large parameter, and to the study of solutions of ordinary linear differential equations by means of asymptotic expansions" (Erdélyi 1956, p.iii). In the introduction to the text, Erdélyi commented on both the origin of the terminology he used and origin of the theory.

Erdélyi stated that Stieltjes used the term semi-convergent, Fritz Emde (1873-1951) used the term convergently beginning series and Poincaré used the term asymptotic series. Further, it is the term of Poincaré that is now generally used. Erdélyi then stated that the theory of asymptotic series was initiated by Stieltjes (1886) and Poincaré (1886).

Copson provided a more detailed history of asymptotic expansions that started in 1730 with the Stirling series, and mentioned work of Euler, Laplace, Legendre (and the very little used term demi-convergente) and noted that the doctorate of Stieltjes was of particular interest. Copson went on to conclude that the modern theory of asymptotic expansions originated in the work of Poincaré.

Unlike the early twentieth century work by Whittaker who firmly situated the work on asymptotic expansions in the development of celestial mechanics, both Copson's and Erdélyi's texts are a mathematical exposition of asymptotic expansions. I do not mean to imply that there were no other publications about asymptotic expansions between Whittaker and Copson/Erdélyi; indeed there were many. However, these two works and this brief discussion of their contents and statements about the origin of their material serves to reinforce my conclusion that it was the work of Poincaré that originated the theory of asymptotic expansions and the main parameters for its subsequent use.

7.3 Concluding Remarks

Stokes' motivation for his work was efficient computation of numerical predictions from theory. These computations allowed for comparison between theoretical predictions and empirical observations which Stokes used to aid in the development of the theoretical understanding of physical observations. Divergent series, for Stokes, led to numerical results that had two different and important outcomes: first, he was the first to understand and provide theory for the physical phenomenon of viscosity, and second, he was able to provide numerical approximations to important integrals in mathematical physics that led to values confirmed by laboratory observations.

Forty years after Stokes, Poincaré was also interested in verifying and answering questions about the physical world. In his case, there were at least two questions of interest in celestial mechanics. The first question was whether or not Newton's gravitational theory was sufficient to explain the observed orbits and the second question was whether or not the solar system was stable.

Poincaré employed asymptotic expansions in the analysis of the differential equations of celestial mechanics as part of his program to determine the stability of the solar system. Poincaré also published an influential and general mathematical paper, independent of application, on asymptotic expansions with careful definitions, proofs and allowed algebra operations. The mathematical paper was published first and provided the theory which Poincaré immediately used in his applied work on celestial mechanics.

Stieltjes' use of asymptotic expansions was motivated solely by mathematical considerations partially related to his interest in finding the values of convergent continued fractions. He provided more heuristic definitions than Poincaré and provided fewer proofs. Stieltjes had a novel and significant method of bounding error in approximations obtained from asymptotic expansions and he used his method to bound the error for asymptotic expansions to a number of important functions, several of which arose in number theory.

Several works on asymptotic expansions during the very late nineteenth century and first half of the twentieth century, detailed above, establish that it was the work of Poincaré that was ultimately the most influential on the development of asymptotic expansions. This is seen in the definitions, the terminology and the methods used in later textbooks.

My conclusion that the work of Poincaré was the most impactful is based on the mathematical reception of each of Stokes', Poincaré's and Stieltjes' work. If, instead, I consider how the use of asymptotic expansions developed by each of these individuals impacted our understanding of physical reality, then Stokes' discovery may well have been the most significant. He was able to identify and provide a theory for the effects of viscosity. This was a newly identified principle in hydrodynamics which Stokes discovered because he had the tool, in asymptotic expansions, that he needed to compute numbers which, when compared with observation, identified a lacuna in existing theory.

Stieltjes, apart from a very brief mention that the Bessel differential equation was useful in many applications from mathematical physics, appeared to be completely uninterested in the physical application of his work. Poincaré's statements about solar system stability, while important and part of which was later codified as the Poincaré recurrence theorem of dynamical systems, were not something which changed underlying theory or led to the ability to understand or use physics in a novel manner. The discovery of 'internal friction' and its effect on the motion of the pendulum by Stokes and thus an understanding of the boundary layer in fluid dynamics had a dramatic impact by this measure.

With regard to the context of the early work on asymptotic expansions that thesis explores, I conclude that the work of Stokes was dramatically affected by the social context in which he worked. During the first half of the nineteenth century, the societal value placed on precision played a large role in the types of experiments that were performed and the instruments that were designed and used. The results of the experiments with these instruments were used to determine values of fundamental physical constants and elucidate the structure of the physical world. The desire for precision and the attendant precise laboratory measurements that were available to Stokes led directly to his desire to provide better numerical computations from theory and this led in turn to an improvement in physical theory.

Further I have provided evidence, based mostly on the direct statements of Poincaré, from which I have concluded that the work of the British mathematicians was oriented differently from that of the French and that we can see that difference when comparing the work of Stokes to that of Poincaré. During the late nineteenth century, British mathematics was less focussed on rigour and more focussed on the practical solution to problems in mathematical physics — problems that arose in mechanics, hydrodynamics, optics or electromagnetism for example. The French, however, were more likely to take a more rigorous, mathematical approach which resulted in careful definitions and development of a more complete mathematical theory before that theory was used in the service of answering physical questions.

Prior to Poincaré there were a series of uses of asymptotic expansion in which convergence was either not addressed or not handled well. In the later half of the nineteenth century, the language to discuss convergence became more widely available. This rigorization of analysis started with Cauchy and was refined by Weierstrass as we saw at the end of Chapter 5. This provides an additional explanation as to the mathematical differences between the work of Stokes and Poincaré.

We have seen three episodes of the appearance of asymptotic expansions during the nineteenth century — episodes which were not well-connected to one another. The work of Stokes, Stieltjes and Poincaré have points of similarity and also there is significant differences between them. They did the most significant work on asymptotic expansions during the nineteenth century.

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